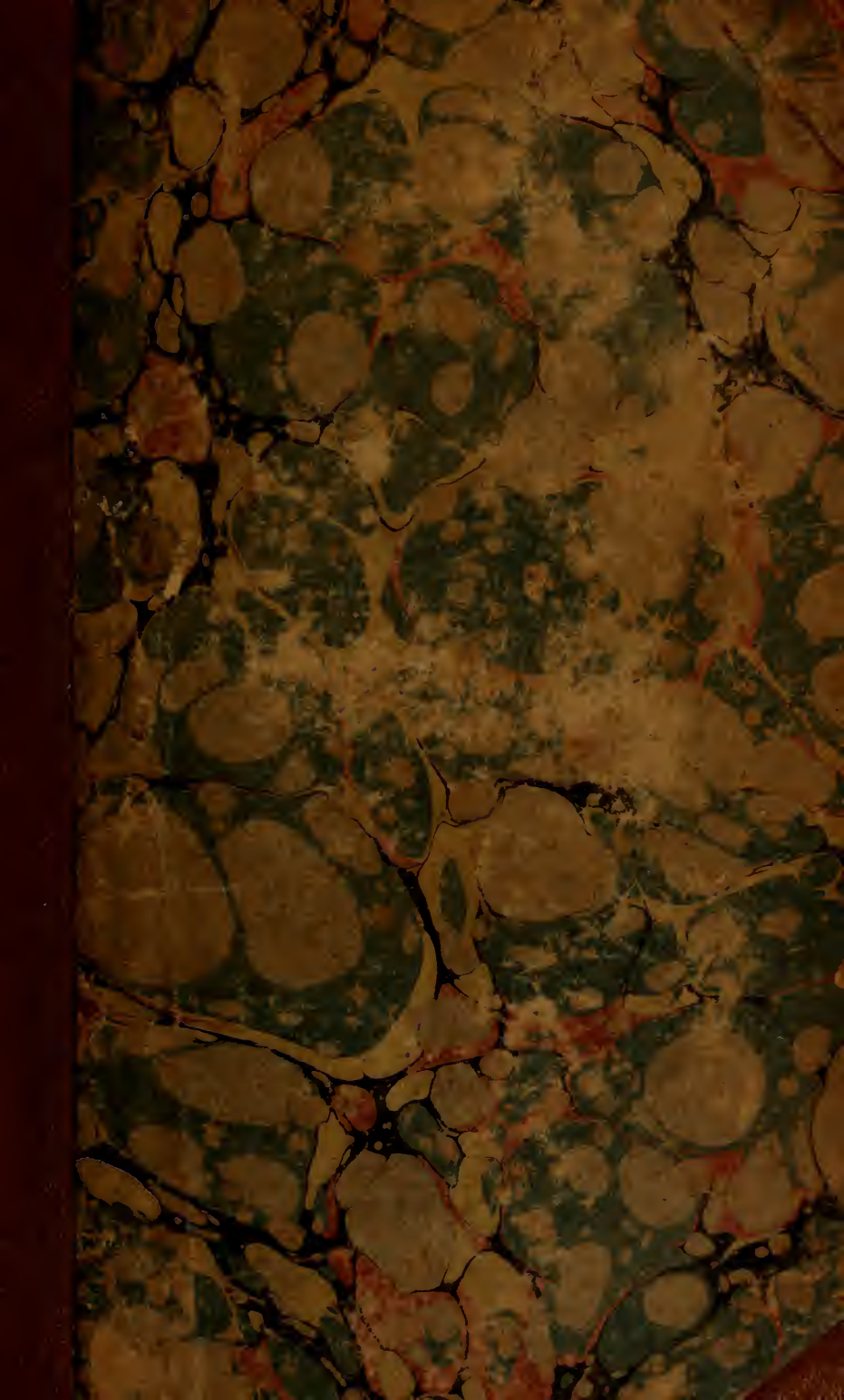
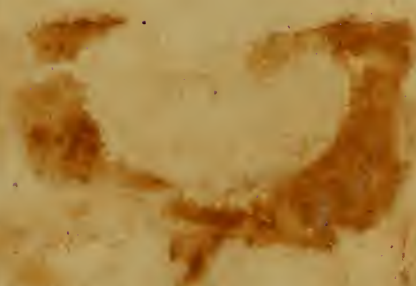












# MEDICAL FACTS

AND

# OBSERVATIONS.

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VOLUME THE SEVENTH.

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L O N D O N :

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M.DCC.XCVII.



Crawford

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# MEDICAL FACTS

AND

## OBSERVATIONS.

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- I. *Practical Observations on the Treatment of acute Diseases; particularly those of the West Indies.* By William Wright, M.D. Fellow of the Royal College of Physicians of Edinburgh, and of the Royal Societies of London and Edinburgh; and Physician to the Forces in the West Indies. Communicated in a Letter to Maxwell Garthshore, M.D. F.R.S. Physician in London; and by him to Dr. Simmons.

To Dr. GARTHSHORE.

DEAR SIR,

IN compliance with your request, I now communicate to you some observations on the treatment of acute diseases; particularly those of the West Indies.

VOL. VII.

B

I shall

I shall begin with the *Typhus*, nervous, ship, or jail fever, as it is differently stiled by different writers.

In a former letter I remarked to you, that the application of cold water externally had been, for some time, practised by Dr. Gregory, Professor of Physic in this University, in cases of Typhus, with remarkably good effect; but he has never carried it to the extent I did in my own case, and in that of others, several years ago\*. Instead of dashing cold water on the naked body, as I did, Dr. Gregory orders the bodies of his patients to be washed with a sponge, dipped in cold water and vinegar, at least twice a day. This operation I shall call the *Lavatio frigida*. The earlier this mode is practised the better; because, in Typhus the patient grows daily worse; for in the second week there is a great increase of fever, and a proportionate loss of strength: but even then Dr. Gregory has found the application of the wet sponge act as a charm; nor have *delirium* or *petechiæ* been considered by him as any bar to the adoption of this remedy; on the contrary, where these have been present, and the

\* See London Medical Journal, Vol. VII. p. 109.



pulse much quickened, he has, by the *Lavatio frigida*, speedily reduced the pulsations from 110 to 90 in a minute, and the delirium and other threatening symptoms have soon after disappeared.

About a fortnight ago, a student of physic, who had been ill for some days before Dr. Gregory was applied to, had, besides a great degree of fever and delirium, numerous spots, or petechiæ, on his breast, belly, and extremities. The *Lavatio frigida* was used on the day the Doctor first visited him, and by next morning the delirium had ceased, and the petechiæ disappeared. The pulse, which on the preceding day had been at 110, was now at 80; and by continuing the application of the wet sponge now and then, the pulse became natural on the fourth day after the Doctor first saw him. Many similar cases might be adduced from the books of the clinical ward of the royal infirmary.

Successful as this method has been in the hands of Dr. Gregory, and some others, besides mine, I am well aware that much caution and judgment are necessary in putting it in practice. In all cases where there are visceral obstructions, cold bathing does much mischief; and in fevers of this sort, with inflammatory diathesis, there



is reason to suspect topical inflammation of the viscera; in this last case, if cold bathing were made use of, the patient would run the risk of his life, and the physician justly lose his character. Other methods of treatment must therefore be had recourse to, and these I will endeavour to point out, from a successful practice in the West Indies, as well as in this country.

In fevers, where there are but slight signs of inflammatory diathesis, mild antimonials, as James's powder, the antimonial powder of the shops, or antimonial wine, in small and repeated doses, with occasional opiates, are generally sufficient to open the pores of the skin, and occasion a gentle perspiration. But where these or the like mild means are of no avail, there is every probability to suppose, that topical inflammation, internally, has taken place.

In cases of this sort I have immediate recourse to calomel, either by itself, or joined with antimonials or opiates. The quantity of calomel I employ is proportioned to the violence of the disorder, and the danger the patient is in. In this country I have seldom exceeded five or six grains of calomel a day; but in the West Indies I have given twenty grains in twenty-four hours with the most marked success.

In

In 1771, Dr. Lysons published his *Essay on the good Effects of Camphire and Calomel in continual Fevers*. In such cases I have found no occasion for the first of these; and Dr. Lyson's success with the latter must have been in cases where there was a morbid and topical affection of the viscera and alimentary canal.

About fourteen years ago, I communicated my method of treating obstinate and acute diseases, in the West Indies, to an eminent physician who had the care of a large hospital in England. He gave calomel, in large and frequent doses, in fevers that resisted the common methods of cure, and found it to answer far beyond his expectations. It sometimes had no other effect than occasioning a copious stool at times; but for the most part it acted as a mild diaphoretic and sedative: a crisis, or favourable turn of the fever, was soon brought about, and the patients speedily recovered.

It seems hardly necessary to mention to you, that in all cases of Typhus there can be but little hopes of success, unless the patients are brought into spacious and well-aired chambers, and are lightly covered with bed clothes.

In the first stage of Typhus, brisk small beer may be given plentifully for common drink, or

water slightly impregnated with vitriolic acid. The strength of the patients should be supported by giving them frequently panada, or gruel, with wine. Attention, too, must be paid to the state of the belly, and of the other emunctories.

Some late authors, who have written on West-India diseases, have roundly asserted, that in tropical countries fevers are not contagious; but whoever has had the care of crowded hospitals, of jails, of ships of war, or of transports full of troops, must have seen numerous and fatal instances of contagion in the West Indies; more especially where cleanliness and free ventilation have been neglected.

From causes of this sort a most fatal and destructive disorder broke out in the West Indies in 1793, and soon after in Philadelphia, viz. the yellow fever. Dr. Rush has classed this disorder with remittents; but every one who has practised in the West Indies, knows for certain, that the remitting fevers of warm countries are not contagious. From Dr. Rush's book, and from the numerous letters of my correspondents, there remains not a doubt, in my mind, of the yellow fever being Typhus, exalted to a great degree of virulence from climate,

mate, situation, and other adventitious circumstances.

The yellow fever has appeared in America at different periods, as we learn from Dr. Lining's paper in the *Edinburgh Essays, Physical and Literary*, Vol. II.; and it was this same disorder that committed such havock amongst the troops, under Admiral Vernon, in 1741.

The commencement of this fever, in Grenada, is dated from May, 1793, soon after the arrival of a Guinea ship from Sierra Leone, the crew of which had been so sickly, that most of the sailors died of the yellow fever, either in the voyage, or soon after the arrival of the ship. It suddenly spread over the other Leeward islands, and from thence was carried to Philadelphia, Hispaniola, and Jamaica.

The first account I received of this fever, was from Dr. James Clark, a physician of eminence in Dominica; his letter to me is dated July 23, 1793, and runs as follows: "I have been harassed night and day, for a month past, by attendance on people ill of the yellow fever. Since its appearance in this island, it has already carried off more than a hundred sailors, new comers, and emigrants. In its progress it has been, and still is, as quick

“ and fatal as the plague ; it often finishes its  
 “ course in forty-eight hours ; but if the sick  
 “ get past the fifth day, they generally recover.”

All the letters I have had from my medical friends agree, that this fever is highly contagious, and that new comers are most subject to receive it ; particularly such as are young, or are addicted to drinking spirituous liquors. Next to these are the nurses and attendants on the sick, who breathe the air in their chambers, or handle their bodies or bedclothes. But such as avoid infected houses, or keep at a distance from people convalescent, are no way subject to the yellow fever. It appears, also, that people of colour, and negroes, are in a manner totally exempted from this disease, except such as are employed as house servants, and fare the same as white people.

The Creole white inhabitants, and others who have long resided in the islands, are, it seems, seldom attacked with this disorder, unless under the circumstances above mentioned. But why the yellow fever should attack some, and not others, can only be accounted for in this way : that in order to receive or resist contagion, men's bodies and minds must be in a particular state ; and that field negroes should not be liable to it



is to me inexplicable. They, however, have their epidemics, from which white people are exempted.

This disorder seems to exert its direful effects on the stomach, intestines, and other viscera in general, but particularly on the liver and gall bladder. Sometimes the lungs are greatly affected; and extravasations have been found in the brain after death.

It is not my intention to delineate the progress and symptoms of this fever; it will be sufficient to say, that bilious vomitings are amongst the concomitant and distressing symptoms of yellow fever; and that what is called the black vomiting generally happens towards the fatal termination of the disease.

I hasten to the medical treatment as practised by Dr. James Clark, and others of my friends, in the West Indies. Dr. Clark, in his letters to me on this subject, regrets his being called so late to the sick in this fever, twenty-four hours having often elapsed before he has seen them: but even at this late period, says he, “ I have  
“ been lucky enough to save three out of four,  
“ or four out of five, of those who had the yel-  
“ low fever.” In cases where he has been called in on the first day of the fever, he assures



me he has seldom lost any one. He first endeavours to purge briskly with ten grains of jalap and ten grains of calomel every three hours. If the vomiting continues, ten grains of calomel, by itself, are given, till stools are procured; and after this calomel, in doses of five grains, with or without opium, every third or fourth hour. In urgent cases he has recourse to mercurial friction, till the violence of the symptoms has abated. “If,” says he, “I can by  
 “any means introduce a sufficient quantity of  
 “mercury into the habit in time, so as to affect  
 “the mouth and gums, I have no hesitation in  
 “declaring that my patient is out of danger.”

Dr. Clark has given sixty or eighty grains of calomel in three days; Dr. Drummond, a learned and eminent physician in Jamaica, has given two hundred grains in the same space of time, besides friction, with strong mercurial ointment, with success.

With regard to bleeding, Dr. Clark tells me he has now and then had occasion to order it in full habits; he has recourse to this, however, but seldom, and then very sparingly. In Jamaica the lancet is now laid aside in the treatment of this disease; as some young men, who were seized with the yellow fever, and blooded on  
 the

the day of the attack, died in a few hours after. The American practice, therefore, will not succeed in the West Indies.

In cases where the strength of the patient is much reduced, the strongest wines, or even brandy itself, must be freely used. Dr. Drummond tells me, that in such dangerous stages of the disease, even when the black vomiting has come on, he has given the pepper medicine\* with success. The use of this medicine is continued till a generous warmth takes place, which must be kept up so long as the debility or the vomiting last; but, in the mean time, the use of mercury must be pushed vigorously, till the mouth is affected, and till there are evident appearances of a resolution of the disorder, and an abatement of the most violent symptoms.

In such stages of Typhus, where there were petechiæ, a difficulty of swallowing, or a sense of choaking; or where aphthæ were present,

\* This is composed of three grains of powder of Cayenne pepper, made into a pill with mucilage, and may be given every two or three hours; but unless the pill is well coated with dough, or white wafer, it will be difficult to persuade the patient to swallow a second dose.

or there was a great irregularity of pulse, I have found the use of æther\* very beneficial.

Hitherto, the black vomiting has usually been considered as a fatal symptom; and a remedy to obviate it has long been a desideratum amongst physicians. To whom the happy discovery of such a remedy, in the capsicum, is owing I have not yet learned; but he merits the thanks of his country, and of mankind!

That a medicine of so hot and fiery a nature, as Cayenne pepper, can be given with safety and efficacy in a disorder so evidently inflammatory, is truly surprising, and can only be accounted for in two ways: first, by supposing that the stimulus of the pepper is stronger than that of the contagion; or secondly, (to use the language of my late ingenious friend, Mr. John Hunter) that it induces a different action in the stomach and first passages.

On the treatment of intermittents I have but little or nothing new to offer: in such cases I

\* For an account of the efficacy of the spiritus vitrioli dulcis in fevers, see a valuable paper, by Dr. Smyth, in the Medical Communications, Vol. I.

have found every advantage from following the advice of my late excellent friend, Dr. James Lind, of Haflar, by giving a large dose of laudanum in the hot fit : this has seldom failed to produce a plentiful and kindly diaphoresis, and the disorder, in general, has afterwards been cured by the Peruvian bark.

Where intermittents have either been neglected or improperly treated, or where the bark, so far from being of service, has served only to load the stomach, or has been rejected, I have suspected that some visceral obstructions existed. In such cases calomel, in small doses, has had the happiest effect, and the patients have generally recovered without any other medicine.

Quartans and double tertians, as well as simple intermittents, are occasioned by marsh miasmata. In warm countries they are frequent, and difficult of cure ; and unless the sick are removed to better air, the disorder will baffle the skill of the most experienced physician. Fevers of this sort, if even continued but for a short time, occasion obstructions of the liver and mesenteric glands, which are too often followed by jaundice, dropsy, and death.

In such cases, after clearing the stomach and primæ viæ, I order mild antimonials, opiates,  
and

and calomel ; by these means the disorder is soon removed, as I have experienced in a great number of cases attended with the most unfavorable appearances.

The common remitting fevers of tropical countries generally yield to the methods prescribed by Doctors Cleghorn and Lind, viz. cleansing the primæ viæ, then giving the bark, wine, and nourishing diet; but if they are attended with bilious vomiting, and symptoms of inflammatory diathesis, calomel, in small doses, (as two grains every three hours) appeases the vomiting, opens the belly, and brings on a gentle moisture on the skin. After this the bark may be tried, but I have often seen the sick recover sooner without it.

Where fiery eruptions, with swelling and inflammation, break out in the mouth and lips; at the decline of bilious remittents, quartans, and other obstinate fevers, Dr. Kirkland justly remarks, that the whole alimentary canal is affected with this sort of erysipelas. To that author I am indebted for the treatment of the patient in this critical and dangerous stage of the disease. Calomel, either by itself, or joined  
with



with mild antimonials and opiates, in small doses, does every thing that can be wished for. If the eruption has continued any length of time, and degenerated into little ill-disposed ulcers and scabs, the *unguentum hydrargyri nitrati* effectually cures them in three or four days.

Remitting fevers, arising from marsh miasmata, are frequently obstinate and fatal. In many cases of this kind the tongue is furred and slimy, and the vomiting incessant, with great head-ach and prostration of strength. Sometimes I have settled the stomach with a decoction of camomile flowers; at other times by saline draughts, taken in an effervescent state; but the most effectual remedy I have ever tried, has been a slight infusion of the *Quassia Polygama*, or Bitter-wood\*; after which the Peruvian bark, or Jesuit's bark of Jamaica†, has completed the cure.

In bilious remittents I have seen a yellow suffusion over the whole body occur in the course of the disease; sometimes in the first stage, but

\* See the account of this tree by Mr. Lindsay, in the Transactions of the Royal Society of Edinburgh, Vol. III. and Medical Facts and Obs. Vol. V.

† Philos. Trans. Vol. LXVII.

more frequently towards the end of the fever, which too often terminated fatally.

In 1785, a gentleman\* at Hampden estate, in Jamaica, was seized with a bilious remittent, attended with constant retching, and vomiting of bile. I was called to his assistance on the fourth day of his disorder; his skin was then of a deep yellow colour, and his urine tinged linen cloth, as in jaundice: the whole of his symptoms indicated extreme danger. My first object was to procure stools by means of stimulating injections, and small doses of the compound powder of jalap; but as the vomiting continued, and the fever remained high, I determined to give him two grains of calomel every two hours. On the following day he was better, but the use of the calomel was continued till the evening, at which time his stomach was settled. He had two copious evacuations by stool; the fever was greatly abated, and there was a gentle moisture on the skin, which I encouraged by small doses of antimonial wine, and watery tepid drinks. After this he recovered daily, but the yellowness of the skin continued some weeks before it wore completely off.

\* Mr. Alex. Thoburn, at present in Scotland.

There are other acute diseases, in warm countries, that are very destructive in their nature ; amongst these, is the *Hepatitis*, or inflammation of the liver. It is either acute or chronic. In acute *Hepatitis* there are strong symptoms of phlogistic diathesis ; and these I endeavour to obviate by a moderate bleeding, gentle laxatives, and diluting drinks. The application of a blister over the part affected, is sometimes useful. If the fever and pain continue, I prescribe small doses of antimonial powder, or antimonial wine, to bring on a gentle perspiration ; should this, however, be not speedily brought about, I lose no time in exhibiting mercury internally and externally, till the disease is conquered : and this I have done with uniform success for twenty seven years ; whereas acute *Hepatitis*, treated by frequent and copious bleeding, too often terminates in *Phthisis Pulmonalis*, or some other fatal disorder.

The chronic *Hepatitis* is very common in Great Britain, and is often mistaken for *Dyspepsia*. Small doses of calomel, (as a grain at bedtime every night for a fortnight) are in general sufficient to remove it.

Pleurisies and acute Peripneumonies are common and fatal diseases in all tropical countries, especially amongst the negroes who live upon estates in the hilly and mountainous parts of Jamaica.

In the cure of Pleurisies, bloodletting is at first requisite; but a repetition of it requires much caution. Profuse and repeated evacuations of this sort weaken the system; and I have seen many instances, where an improper use of the lancet in such cases has been succeeded by general debility, pulmonary consumption, and dropsy. In these diseases, after one, or at most two moderate bleedings, I direct the belly to be opened by clysters, or some gentle laxative; give nitre dissolved in the patient's common drink, and advise a thin spare diet. A blister applied to the side affected generally gives great relief. But if the fever is considerable, and the pain acute, I order from three to six grains of antimonial powder every two hours, till a plentiful sweat takes place, which I encourage by a liberal use of warm tea, or water gruel. If small doses of the antimonial powder have not the desired effect, I give ten, fifteen, or twenty grains

grains for a dose; nor am I afraid of exciting full vomiting, either in Pleurisy or Peripneumony; on the contrary, such doses have proved highly beneficial.

When the disorder has resisted these means, I have ordered, with great success, calomel, in large and frequent doses, as long as the violent symptoms continued.

Pleurisies and peripneumonies are often epidemic amongst the negroes in Jamaica, and attended with a remitting fever. Full vomiting is here particularly useful; in the exacerbations twenty-five or thirty drops of laudanum take off the spasm, and the bark secures the patient from a return of the complaint.

I might have mentioned *splenitis* and other internal inflammations, but as they give way to similar management, I proceed to treat of the dysentery.

The dysentery has in every war carried off more of our troops in the West Indies, than all the other diseases of that climate. It is a melancholy truth, that this fatality is greatly owing to the folly and intemperance of soldiers and sailors, and not to the climate, which



has been blamed for it. Drinking to excess of new and bad rum, destroys the powers of the stomach, and debilitates their strength : they are either attacked by some violent inflammatory disorder, or are liable to receive infection from human bodies, or from marsh miasmata.

Europeans suffering under dysentery, in the West Indies, have more or less of remitting fever : in such patients bleeding, if at all necessary, ought to be had recourse to very sparingly. Negroes ill of dysentery, or other acute diseases, admit of a more free use of the lancet. In ordinary cases, an emetic of ipecacuanha, afterwards a dose of rhubarb and calomel, and an opiate at bed-time, generally carry off the disorder.

In epidemic dysenteries, attended with great prostration of strength, and other symptoms of putrescency, I am solicitous to purge off the offending matter in the alimentary canal, and afterwards to correct the disposition to putrescency : for this purpose I prescribe a strong decoction of tamarinds; in two pints of which I order two ounces of purging salt to be dissolved : an ordinary tea-cup full of this is directed to be taken every three or four hours, till it has operated

operated plentifully by stool; after which, at bed-time, I give an opiate. On the following day the decoction of tamarinds, without the salts, is given; or the sick are allowed to eat preserved tamarinds, as they think proper.

In cases where this method has failed of success, I have had recourse to a mixture of vegetable acid and purified sea salt, an account of the preparation and good effects of which I several years ago communicated to the American Philosophical Society, who have inserted it in their Transactions\*: it is composed of lemon or lime juice three ounces; of sea salt purified an ounce, or as much as the acid will dissolve; of any simple distilled cordial water one pint; and of loaf sugar a sufficient quantity to sweeten it: of this a wine-glass full may be given to adults every two, four, or six hours.

A most respectable author defines dysentery to be a fever of the intestines, and for the cure of it prescribes antimonials and opiates, which in slight cases I have known to answer. This idea of the disease comes very near to my own;

\* See Transactions of the American Philosophical Society, Vol. II. 4to. Philadelphia, 1786; and London Medical Journal, Vol. VIII. p. 97. 8vo. London, 1787.

but when dysentery is attended with phlogistic diathesis, the fever is rather the *effect* than the *cause* of the disorder. Dissections of such as have died of dysentery, have evidently shown, that inflammation, and consequent gangrene, had taken place in the smaller intestines, as well as in the colon.

In dysenteries where the fever has been considerable, the tongue dry and parched, the gripes severe, and the stools very frequent, with scarcely any thing else than blood or mucus, I have prescribed, with good effect, calomel, in doses of five grains, every six hours, till a copious stool or two has been procured; and afterwards in smaller doses, with occasional opiates, while the fever and gripes have continued.

Autumnal dysenteries in this country have generally given way to some one or other of the correctors I have mentioned above; but particularly to an infusion of *Quassia Polygama*\*, or Bitter-wood; after which I have prescribed the Peruvian bark to strengthen the system.

\* There is no such thing in the shops as *Quassia Amara*; it is the Bitter-wood or Bitter-ash that is imported, and answers every purpose, perhaps better than the *Quassia Amara*.—Vide Medical Facts and Obs. Vol. V.

In the treatment of the different diseases mentioned in this paper, you have seen the liberal use I make of calomel. I have contented myself with candidly relating to you the effects I have experienced from it, without attempting any theory of the mode in which these effects are produced. I think it necessary, however, to observe to you, that freely as I have administered calomel in different acute diseases, I have seldom, if ever, been surprised with a sudden salivation. I indeed have paid daily attention to the state of the mouth and gums, and as soon as I have observed the latter spongy, and that the tongue was beginning to be moist about the edges, I have desisted from the farther use of calomel; because I was then certain that a resolution of the disorder was begun, and that my patient was out of danger.

In answer to your question, how early I got the first hint of the use of calomel in fevers? I answer, it was my good fortune, for many years, to enjoy the friendship and confidence of the late Dr. Lind, of Haßlar; and it was from his conversation, and the information contained in his excellent work on the diseases of warm cli-

mates, that I learnt the East-India practice of giving mercury in inflammations of the liver, and the success with which the late Sir John Eliot had treated visceral obstructions by the same remedy; all which I knew so early as the year 1760. But it was not before 1764 that I began to give calomel in so free a manner as I have done ever since, not only in hepatitis, but in all the other acute diseases I have treated of; and I extended its use from reasoning in my own mind, and from analogy. I have never had cause to repent of the further trials I made; but, on the contrary, have had reason to consider this practice as the happy means of saving the lives of a great number of people.

I think it right to add, that Dr. Drummond, of Westmoreland, in Jamaica, whom I have already had occasion to mention more than once in the course of this letter, began to administer calomel in fevers and pleurifies as early as I did, though without our having had any communication on the subject with each other. I have since found that he learned the use of it, in such cases, from Dr. Smith, a physician at Savannah le Mar, who was in the habit of giving it, in doses of twenty grains, in acute diseases, with great success.

These



These observations are extended to a greater length than I at first intended. After all, you must consider the whole only as hints for the treatment of acute diseases, and if you are of opinion that they will be useful, you have my consent to make them public.

I have the honour to be,

With the greatest esteem and regard,

Dear Sir,

Your most obedient servant,

WILLIAM WRIGHT.

*Edinburgh,*

*December 10, 1794.*

*II. Facts relative to the Origin of intermittent Fevers. Communicated in a Letter to Dr. Simmons, by Thomas Beddoes, M.D.*

I DO not know whether it is the prevailing opinion among medical philosophers, that Dr. Cullen was mistaken in referring the origin of intermittent fevers to effluvia from marshes exclusively. Many physicians believe that they may be excited by other causes; and of this there seems to me to exist in different books such satisfactory evidence, that I should not consider the following cases as worth recording, if the circumstances had not been somewhat peculiar, and at the same time perfectly distinct.

On February 2, 1795, I ordered the first medicines for Miss Sweeting. On the 25th of January, in the evening, she had been chilled in going home; she was unwell the next day, and for several days preceding the 2d of February, had had a daily cold paroxysm, followed by heat and copious perspiration.

I saw her one day in the cold stage of her paroxysm, and it was impossible to imagine more  
distinct

distinct shuddering and shaking. In the interval she was lively, and had no complaint; but she was beginning to lose her healthy look. Her complaint was easily removed; on the 18th of February, however, she was threatened with a relapse, and on the 19th had a slight paroxysm; but her former medicines at once put an end to the disease.

She lived in Rodney Place, and had not been in the valley for a long time. She was at Bristol just a week before the evening she was chilled, but in the intermediate time had never once been off Clifton Hill.

February 4, I was desired to see Mr. Thomason; he lived at the Hotwells, near the river. I found him in the hot stage of his paroxysm; this was succeeded by a profuse diaphoresis, and that by a complete intermission. The bark freely taken, with some additions, prevented any return of his fever. About a week before I saw him, Mr. T. had been making some magnetical experiments in a large cold room, during which he had felt uncomfortably chilled; the following day he shivered and felt feverish at times, and he had not been free from these feelings for twenty-four hours together,

till his complaint was cured by the medicines I have mentioned.

Mrs. — had closely attended a sick relation at the Hotwells. In the beginning of February she changed her lodgings; the room in which she sat the first day after her removal had been lately washed, and was still damp; she felt indisposed before bed-time. On the morning of the third day I saw her, she had been chilly, was now in the hot stage of a febrile paroxysm, and afterwards fell into an excessive perspiration; she had then a perfect intermission, and took medicines similar to those above mentioned. She seemed recovering as usual, when she imprudently took a cathartic medicine, which operated with great violence. Her fever now returned; it assumed the form of a tertian, and proved very obstinate.

In these cases, where the disorder came on during a severe frost, it is impossible to think of marsh miasmata. Indeed, the feelings and observations of each patient so clearly point out the time and manner in which the disorder took place, that I presume no doubt can remain when both circumstances are considered.

Opportunities

Opportunities of making observations in physic, superior to all cavil, occur so seldom, that thought the present ought not to be neglected.

*Clifton,*

*March 7, 1795.*



III. *Observations on the Nature of Corns, and the Means of removing them. Communicated in a Letter to Dr. Simmons, F. R. S. by Mr. Anthony Carlisle, Surgeon to the Westminster Hospital.*

*To Dr. SIMMONS.*

SIR,

**I** TAKE the liberty of laying before you the following observations upon the nature and treatment of corns, and other similar excrescences of the cuticle, as a subject not unworthy the attention of the medical profession; because want of skill in the treatment of trifling diseases is very justly an occasion of reproach  
against



against regular practitioners, and it also leaves a fair opening for empirics.

I have been the more desirous to make these observations public, from having often found them useful to myself in practice; and I have also reason to suspect that they are not generally known to medical men. In addition to these arguments, I hope that the succeeding remarks may be found the means of improving the cure of some painful and troublesome diseases, which although of little importance in the estimation of many persons, yet are difficult to remedy without a previous knowledge of their nature from regular data.

The cuticular covering of the human body appears to be lifeless and inorganic\* upon its outer surface. It is connected by vessels to living parts upon its under surface, and is probably vascular when first formed. It is composed of lamina, of different degrees of thickness, and is superficially divided into distinct masses of various forms in different parts of the body; for example, the scales of the cuti-

\* By inorganic, I here mean destitute of that arrangement of vessels, and connection with the acting parts of the body, which would otherwise have subjected it to the same conditions as the living parts.

cle which are shed from the legs are unlike those from the palms of the hands.

There are holes through the cuticle for the matter of perspiration to pass out, and other openings for the transmission of hairs. The former are similar to the holes through the inorganic shell of an egg, and do not appear to be under the influence of life; the vessels leading to these holes are, however, susceptible of various changes. It would seem that the processes of the cuticle which pass into these holes in the true skin, form an open canal between the exhalent artery and the outward air. It does not appear that this external covering of the body is at all acted upon by the absorbing system of vessels.

The cuticle is capable of undergoing many changes whilst it is attached to the living body, without its being afterwards thrown off. It may be distended with water to a very unnatural thickness, as on the hands of persons working in warm water; but will recover its former state again in a few hours, after leaving off the use of the water.

The cuticle may be scorched with fire, or coagulated, so that it never resumes its natural appearance

appearance again, and yet it shall continue to cleave to the living body.

The cuticle seems also capable of putrefaction whilst upon the living body.

In persons who are liable to much perspiration about their feet, and who are at the same time not in the habit of washing them often, I have many times observed the thick cuticle upon the under surfaces of the toes, and at their junction with the sole of the foot, in a very putrid state, dissolving and rotting until the cutis itself was exposed so much as to bleed.

The putrefaction of this substance is accompanied with a peculiar smell, and not the matter of perspiration, as is vulgarly supposed.

The connection between the living body and the cuticle may be destroyed by a variety of stimulating applications, and even particular states of the body produce untimely separation of this covering; for example, œdema, some fevers, violent courses of mercury, &c. When the separation of the cuticle from the living body is produced rapidly, a quantity of the serum of the blood is thrown out between it and the living parts, as in the cases of scalds, burns, blisters, &c. This is a curious system of defence. When the separation of the cuticle is effected

effected more slowly, it sometimes happens that the new layer of cuticle is formed before the old one loses its hold, and in this way the substances of the two will be interwoven together; the accumulation of successive layers clinging together in this manner forms a corn. The application of moderate pressure seems to be conducive both to the growth of new cuticle, and to the adhesion of the new with the old.

A corn will often originate from a small blister, produced by severe pressure, more especially when the blister is accompanied with much inflammation of the skin beneath. In this case the coagulating lymph is thrown out into the blister, and becomes the basis of a corn.

The natural process is for the old cuticle to be continually shed in the form of scales as soon as the new is formed.

When the cuticle is suddenly removed from the living parts, as in the cases of blisters, &c. before mentioned, the vessels of the skin throw out a quantity of the serum of the blood; they also throw out the coagulating lymph, which appears to be the substance from which the cuticle is formed. When the parts beneath the cuticle are essentially injured by the stimulus which separated the cuticle, it often follows that

the lymph is thrown out very irregularly, forming ridges in some places, and unnaturally thin coverings in others. These phenomena appear to depend upon the unequal actions of the living parts below. A strong stimulating application laid over the whole extent of the sore is a remedy for this.

The cuticle, when first formed, is of a curdly cheese-like substance, and the thickness of each layer depends upon the quantity of this substance thrown out at one time, which is very irregular.

The natural cuticle is a mask of the surfaces of the living body to which it is attached.

The forming of cuticle upon a naked living surface, appears to be hastened by the application of some substances which coagulate the animal mucilages, such as æther and alcohol. Stimulating the living parts is also often necessary, in order to produce that degree of action in the vessels which attends the throwing out of the coagulating lymph. The substance of the cuticle may be dissolved by the application of Caustic Alkali, or *Aqua Kali pur.*

The most usual occasion of corns is the application of moderate and long-continued pressure. In this case a process is set up to defend  
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the living sensible parts against the injury, by thickening the insensible covering or cuticle. In this instance, however, the natural means made use of do not answer the end, and the reason might be given. The pressure is commonly made at first upon a small surface; the thickening of the cuticle increases this pressure, by diminishing the space between the pressing substance and the living part. Still, however, new layers of cuticle are formed, and the true skin begins to be removed out of the way by absorption, thus allowing the diseased mass of cuticle to sink below the level of the living parts; proceeding upon this plan a cone of cuticle is formed with its apex protruded among sensible substances, so that upon pressing or moving the part considerable pain ensues.

The difference of the forms of these bodies has given rise to a variety of vulgar names, which it is not necessary to enumerate, because these observations are intended to apply to every possible species of the disease, upon natural principles. There are, however, two kinds of corns, which differ in many respects from each other; the one is called the hard, the other the soft corn; the latter is always situated in a per-

spiring part, which seems to be the occasion of its peculiarity.

It must be understood, that although the substance of a corn is composed of the same kind of matter as the cuticle, yet it is not modelled in the same manner; the layers are thicker, are often cheefy in parts, and generally admit of the evaporation of a part of the water naturally combined with them; so that they form hard bodies, much more brittle and inflexible than the natural cuticle.

Sometimes inflammation takes place in the skin at the root of a corn, and the diseased cuticle is thus dislodged by a small abscess: this is the natural cure.

I thought it right to premise thus much of the natural history of this part of the body, in order that the principles upon which every remedy to be hereafter advised, may be clearly understood. The diseased state of this substance is not always radically cured without some trouble, and different means must be applied in different cases, suitable to the circumstances of each. The matter of the cuticle itself, which forms this disease, is a substance of little consequence; but in certain habits of body the extreme parts, where corns are usually to be found,

found, are often much injured, both by the disease and by injudicious attempts to remedy it; the extreme parts of the body being, in general, less under the command of medical treatment than the other parts of the body.

There are many rational methods of curing the disease called corns. The most common is by cutting or paring them; this never answers any farther purpose than temporary relief, because the remaining cuticle is so diseased, that it does not recover the power of shedding the old external lamina, whilst new layers are forming underneath; and from this cause the corn will be soon renewed. Another objection to cutting is the impossibility of completely paring the corn when its under surface is conical or irregular, so as to leave only one layer of cuticle remaining, which ought to be done, else the corn will grow again. In some particular corns, however, the connection between the diseased substance and the skin is so slight, that a line may be perceived where they unite, and with a blunt pointed instrument the corn may be separated and completely turned out; but this is only in some old corns.

One method of curing corns radically is by dissolving them. After having softened them

by steeping the part in warm water, apply the liquid caustic alkali, or *aqua kali pur.* upon a piece of sponge, keeping it on about a quarter of an hour, or until a little pain is felt. A portion, if not all the corn, will be so dissolved by this fluid, as to be easily rubbed off with a coarse cloth, and this process must be repeated until all the diseased cuticle is removed. The management of this application requires some address, and often considerable patience and perseverance; so that many people are not disposed to adopt it; and, besides, if it be not followed up until the cutis itself is naked, there is no certainty of our having entirely destroyed the diseased structure.

In the above manner, hard and thick nails, which are producing inconvenience, may be also softened, and the troublesome part removed.

Another method of cure, which is, however, far more tedious and inconvenient, is by the application of pieces of adhesive plaster, spread upon leather, each piece having a round hole cut in it, so as to admit the corn to pass through. In this manner pieces of the plaster are placed over each other, until they rise above the level of the top of the corn; and a shoe is to be worn of a bigness sufficient to contain the whole,  
so

so that the pressure shall fall, by means of these plasters, upon the skin immediately surrounding the bases of the corns. The consequence of this is the protrusion of the sunk root of the corn, and it is shed like natural cuticle. It requires that these plasters be kept constantly applied for the space of from three to six weeks, before they answer the purpose. Very adhesive plaster, spread upon firm buckskin leather, seems to suit this end exceedingly well. This method is always effectual, if judiciously applied and persevered in; and it is not attended with any hazard of ill consequences.

A third method of curing corns is by the application of a blister. The corn should be first pared as close as possible, and a piece of blistering plaster applied, big enough to cover the base of the corn, and to pass a little way over the sound cuticle. The blister requires, in general, to remain upon the part for twenty-four hours, and should be continued longer, provided it neither produces vesication nor pain. The common blistering plaster is not strong enough; it requires the addition of a little Euphorbium. If much inflammation attends, it may often be removed by the application of a simple milk and bread poultice. This method



is particularly objectionable in old persons, and in bad constitutions, because violent irritation produced in the extremities of the body, in such cases, does not always terminate well.

The method of cure by blistering is, however, one of the most effectual and expeditious, and perhaps the only one for the soft corns.

At the conclusion of these observations, it may be necessary to remark, that I have spoken of the effects of all the foregoing remedies, as they have fallen under my own experience; but it is no more than just to add, that much of the reasoning, as well as the practice, was suggested by the late Mr. John Hunter, and that the whole plan is founded on principles adopted in the Hunterian school.

*May 24, 1795.*

IV. *Some Observations relative to the Angustura Bark. Communicated in a Letter to Dr. Simmons, by Thomas Masterman Winterbottom, M. D. Physician to the Settlement at Sierra Leone.*

**I**N compliance with your request I send you a short account of my experience relative to the Angustura bark, with a few of the cases in which I have employed it. A long time is, in general, required before the merits of a new medicine can be duly ascertained; for while some extol it to the skies as a cure for every disease, others are equally fond of depreciating the powers which it actually possesses. Experiments frequently repeated are therefore necessary, before we can form a just estimate of the effects of any remedy. In the present instance I shall scarcely be accused of precipitancy, or of giving an opinion drawn only from a few imperfect cases. During my stay in Sierra Leone I have used upwards of fifty pounds weight of the Angustura bark in powder; and from every trial hitherto made of it, I think myself warranted

ranted to recommend it as an active and valuable medicine.

In several comparative trials made with the Angustura and common Peruvian barks, in regard to their febrifuge and tonic powers, I have always found the former to be equally efficacious with the latter, and that frequently in smaller doses. In those cases, however, where it is necessary to give this medicine in substance, and in large doses, as in the remittent fever, with a view to put a stop to the return of the paroxysm, the Angustura bark could not always be given for a sufficient time, without exciting nausea; but where this effect was not produced, I have trusted the course of a remittent fever to the Angustura with the same confidence as to the Peruvian bark, which last is usually considered as a specific for that disease. It must, however, be observed, that in the cases of fever where the Angustura bark was employed, the doses were perhaps larger than might be absolutely necessary; but the fever of this country is usually so rapid in its progress, that if the paroxysms be not soon put a stop to, the remissions become obscure, or scarcely perceptible, and the patient is suddenly carried off. I did not venture, therefore, to use it in smaller

ler doses than what I had from experience found necessary to be given of the Peruvian bark; nor did I consider my patient as secure unless he had taken, during the time of a remission, as much of it as his stomach would bear.

Towards the decline of a fever, when debility is the chief symptom, I prefer the use of the Angustura bark, in infusion, to a farther continuance of the Peruvian bark; this change is generally very agreeable to the patient; the infusion sits easy on the stomach, and is attended with the most beneficial effects in restoring the strength and appetite. I have also found Angustura bark very effectual in the cure of intermittents: but as these most commonly occur among the seamen and Nova Scotian settlers, who are not easily induced to take a disagreeable medicine for any length of time, I have been almost always obliged to substitute the arsenical solution in place of the bark.

## CASE I.

July 15, 1792. Mr. A. aged 38, of a fallow complexion, with light brown hair, was,  
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on the evening of the 13th, attacked with a remittent fever, the symptoms of which were very slight, and went off towards morning, but returned at night with greater violence; he then took a dose of James's powder, which vomited and purged him several times during the night. At present, (four, p. m.) he complains of pain in the head, especially over the eyes, and great depression of strength; his pulse is 100, and quick; his skin clammy, and rather hot; he is affected with nausea, and inclination to vomit, on sitting up or attempting to walk: tongue foul, much thirst. He was ordered to take at bed-time a draught, with Tinct Opii, gutt. xxx. Sp. Nitr. d. gutt. xl. Aq. font. 3fs.; and Pulv. Cort. Angust. 3fs. every hour, beginning early in the morning.

16. (Ten, a. m.) He has passed rather an uneasy night, and seems very irritable, being easily affected by the slightest noise, &c. Skin rather cooler; pulse 100; other symptoms as before. The powder and draught to be repeated.

17. Had an exacerbation of fever yesterday at two, p. m.; pulse 120, and quick; skin very hot and dry; pain of the head much increased; great restlessness and anxiety. Took  
yesterday



yesterday fix drachms of Angustura bark in powder; the medicines to be repeated.

18. Ten, a.m. Has passed a very restless night; had a return of fever yesterday, at the same time as the day before, with nearly the same symptoms, but which went off towards evening, with a clammy, foetid perspiration. He is rather costive; pulse 96; much nausea was produced by the medicine, of which he took yesterday an ounce, and retained it, though with great difficulty. Twenty grains of *Kali pp.* were directed to be added to each dose of the powder, and a large spoonful of lime-juice to be taken immediately after it.

Six, p.m. The paroxysm returned much later this afternoon than usual, and was very slight. At present, his skin is cool and moist; his thirst less; head-ach abating; pulse 100, and pretty soft. He vomited after one dose of the powder, but has retained near an ounce of it to-day; feels much less nausea since he has used the effervescent draught. Powders and anodyne draught to be repeated; to take also three aloetic pills at bed-time.

19. Has passed a tranquil night, but without sleep; feels much easier this morning; a slight pain still remains over his eyes; his tongue  
is

is moist, but he has a disagreeable taste in the mouth; his skin is cool and moist: he had one stool from the pills. The medicine being taken with great difficulty, it was ordered to be omitted after this day. In place of it he was desired to take half a drachm of Peruvian bark, in powder, every four hours, beginning in the morning. The draught was repeated.

24. He has had no return of fever since the 18th, and has no complaint at present but of languor and want of sleep, to which he is often subject when in health. His appetite returns.

## CASE II.

Mr. N. aged 40, of a dark complexion, and of a robust habit of body, was taken ill of a fever at the same time as the last patient. The chief symptoms in Mr. N.'s case were great prostration of strength, with restlessness and anxiety. His pulse, which in health beat only sixty-four times in a minute, and was remarkably full and strong, during the paroxysm never exceeded eighty-six in a minute. He took every day, for a week, ten drachms of the powder of Angustura bark, without experiencing the

the least nausea. The Angustura bark appeared to stop the recurrence of the paroxysms sooner in this case than the former; and the patient's strength returned much more speedily.

From the beginning of his illness till some time after the perfect recovery of his health, his urine was of a very dark colour, like the grounds of coffee.

### CASE III.

March 9, 1794. J. Fargrain, sailor, aged 30, of rather a fallow complexion, has felt, for two days past, much languor and lassitude, with pains of the head and back, and frequent slight chills, which usually came on towards evening, and abated in the morning. At present he has an acute pain over his forehead, and a dull pain or heaviness across his loins; much giddiness on standing in an erect posture; tongue rather white; a bad taste in the mouth; much thirst, and a considerable depression of strength. He was directed to take in the evening, *Antimon. Tartarizat.* gr. ij. and *Pulv. Ipecac.* ℞j.; and at bed-time, *Tinct. Opii.* and *Vin. Antimon.* āā. ʒss.

10th. (Ten,

10th. (Ten, a.m.) The vomit operated, and evacuated much foul bilious matter. He passed rather a restless night, but feels considerably relieved this morning; pulse 86; skin moist and pretty cool; a slight pain still remains over his eyes; some thirst; pain of the back almost gone. He was directed to take half a drachm of powder of Angustura bark every hour, and at night repeated the tincture of opium and antimonial wine.

11th. (Ten, a.m.) Had an exacerbation of fever last night, attended with severe pain of the head, and felt much anxiety and restlessness during the night. At present, pulse 100, and soft; skin hot, but moist; much thirst; he had one stool this morning. He took yesterday six drachms of the Angustura bark, but vomited after the last dose; he was directed to take a drachm of the powder every hour. The tincture of opium and antimonial wine were repeated at bed-time.

12th. He has rested pretty well all night, and felt no return of fever; had four stools yesterday, and two in the night. Pulse 80, soft, and rather full; skin cool; tongue pretty clean, but dry; thirst abated; a slight headache still remains; urine rather high coloured and

and transparent. He took yesterday an ounce and a half of Angustura bark; the other medicines were repeated at bed-time.

13th. He has passed a restless night, attended with great anxiety, but has had no return of fever. At present, pulse 88, soft, and pretty full; skin cool; has still a slight pain over his eyes; he has some return of appetite, and is able to sit up a little in bed; his urine is still high coloured, and without cloud or sediment. He took yesterday an ounce and a half of Angustura bark; the other medicines were repeated.

20th. He has continued the use of the Angustura bark since the 13th, but in doses gradually diminished. He has no complaint at present but from weakness. His urine is high coloured as before. He was desired to take half a drachm of Angustura bark three times a day, for a few days longer.

#### CASE IV.

Mr. P. aged 45, of a fair complexion, with light brown hair, was seized, on the 23d of March, 1794, at five, p.m. with fe-



vere pain over his eyes, giddiness, nausea, and retching. He took immediately *Antimon. Tartarifat.* gr. ij. and *Pulv. Ipecac.* ℥j; and, at bed-time, thirty drops of tincture of opium.

24th. (Ten, a. m.) The emetic operated several times; he passed the night without sleep; feels very languid at present, and complains of a severe pain of his head and back; his skin is hot and clammy; his pulse 98, full and soft. He complains of great giddiness and increase of head-ach on raising his head from the pillow. He was directed to take two scruples of the powder of *Angustura bark* every two hours; and at night to repeat the opiate

25th. He rested very ill last night, which he attributed to the opiate: symptoms in other respects much the same as yesterday. He complains of great thirst; his tongue is very clammy, and covered with a white fur; skin hot but moist; pulse 98, and pretty soft; body open. He took yesterday ten drachms of *Angustura bark*, but vomited after the last dose; the opiate to be omitted.

26th. He had some sleep last night; the pain of his head is now less severe; the pain of his back is removed; he had three loose stools during the night; pulse 90, and soft;  
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the debility and giddiness still very great. He took yesterday ten drachms of Angustura bark, and had some nausea from the last dose, but without vomiting.

27th. He slept pretty well last night, and feels himself considerably better to-day. His head-ach is almost gone; pulse 84, soft, and pretty full; skin cool; tongue moist and clean. He has much giddiness on attempting to sit up, and begins to nauseate the powder; he took, however, an ounce of it yesterday, and retained it: half a drachm of it was now directed to be given only every three hours.

30th. He complained only of debility, and his appetite was pretty good; he still continued, however, to take the Angustura bark for some days longer.

In dysentery I have not had so many opportunities of trying the effects of Angustura bark, as in fever, for though dysenteries sometimes occur here, they are not very frequent, nor, in general, obstinate. In those cases, however, where the symptoms still continued to harass the patient, after the bowels had been sufficiently opened by saline and mercurial purga-

tives, the Angustura bark never failed to put a stop to the complaint. In these instances the powder was most commonly used, in doses of from one to two scruples, and repeated three or four times a day, according to the circumstances of the case. Sometimes an infusion of the bark was given, prepared by macerating half an ounce or six drachms of it powdered, in a pint of water for twelve hours, in a gentle heat.

Diarrhœas occur frequently in this place, and are often of a very obstinate nature, particularly when they attack those who have been much debilitated by previous disease, or who, during their convalescence, have indulged in the use of improper food; or, as is more commonly the case, when they attack those who cannot procure a diet sufficiently nourishing. In all these cases I have seldom or never found the Angustura bark to fail in putting a stop to the discharge. When the diarrhœa has continued for any length of time, and the stools have been frequent, small, and attended with some degree of tenesmus, I have found it useful previously to check the disease, by exhibiting from fifteen grains to a scruple of *Pulv. Ipecac.* This was done, not in order to evacuate any acrid matter from the stomach, but with a view of  
exciting

exciting an antiperistaltic motion of the stomach and intestines, or of counteracting their irregular action, which had been probably kept up through habit. A similar effect is frequently produced in long-continued vomitings, which, after resisting for a length of time the medicines usually employed, are often speedily removed, by briskly exciting the proper motion of the intestines.

When the emetic has produced its full effect, the *Angustura bark* is to be given in doses suited to the patient's age and situation; and such is the effect of this mode of treatment, that in the course of a few days it commonly becomes necessary to administer a gently opening medicine, to prevent costiveness from ensuing.

## CASE V.

J. Allen, aged 30, of a fair complexion; has been near a year in this country, during which period he has had two or three severe attacks of fever, which have reduced his strength very considerably.

Sept. 22, 1793. He has been affected for four days past with severe and constant griping pains round the umbilicus, attended with fre-

quent purging and tenesmus; his stools are small, frothy, and chiefly composed of blood and mucus; pulse 100, quick, but rather soft; skin tolerably moist. He labours under great debility, and is very anxious respecting his recovery. He has taken, since the first attack, *Tinct. Opii & Vin. Antimon.* āā. gutt. xxx. and a pill, containing three grains of calomel. All these were repeated this evening.

23d. (Ten, a. m.) He has passed a very restless night; and has had several stools, which were rather larger than usual, and of a more natural colour; the tenesmus is abated; his pulse 112, and quick; his tongue dry and brown.

He was directed to take half a drachm of Angustura bark three times a day. The tincture of opium and antimonial wine were repeated at bed-time.

24th. He had some rest during the night; and several stools without tenesmus: the griping pains are now entirely gone; his skin is cool; his tongue moist; his pulse 80, and soft. The tincture of opium and antimonial wine were repeated.

25th. He rested pretty well last night; he has had only five stools, and these were without griping or tenesmus. His weakness and anxiety are not much abated; pulse 120, and quick, occasioned



fioned by his sitting up and walking a little about the room. The same medicines were repeated.

28th. He had only two stools to-day, which were perfectly natural. The pains of the bowels and other dysenteric symptoms seem to be wholly removed.

October 2d. Debility is now the only remaining symptom of disease; he has, in general, two natural stools every day, and his appetite is returning. He was desired to continue his medicines some time longer.

## CASE VI.

July 14, 1792. Capt. ———, has resided a considerable length of time upon this coast, being chiefly employed in trading up small rivers and creeks, by which his health has been greatly impaired. About two months ago he was seized with very severe dysenteric symptoms, which have continued to harass him ever since; he has only been able to procure a few doses of salts, but was somewhat relieved by them from the griping and tenesmus. At present he appears very much reduced; his countenance is fallow; his stools are very frequent,

small, and frothy, and discharged with pain. He has frequent griping during the day, especially after eating; pulse feeble, and rather quicker than natural; skin pretty cool. He was directed to take in the morning Sal. Cathart. Amar.  $\text{ʒi}$ . in divided doses, and after they should have operated, to take half a drachm of Angustura bark, in powder, three or four times a day.

I did not see him again till some time afterwards, when he told me that he enjoyed better health than he had done since he came upon the coast. His complaint, he said, left him after he had taken the powder three days, but he continued the use of it some time longer.

## CASE VII.

January 1, 1794, Mr. B. aged 40, of a fallow complexion, with black hair, has been affected for six weeks past with severe griping pains, chiefly round the umbilicus, which are considerably increased after eating. He has also a constant tenesmus, and goes to stool not less than twenty times a day; the discharges are small, mucous, and generally tinged with blood: his countenance is very unhealthy, pale,  
and

and dejected; pulse 90, and pretty soft; skin cool; much thirst. He resided a long time on the river Gambia, where he was affected with a remittent fever, which, after debilitating him considerably, changed to an intermittent; this latter was also very troublesome for a long time, but quitted him before his arrival at Sierra Leone. The present complaint began with severe pain of the stomach, from which he was sometimes relieved by opium and warm cordials; and the pain gradually descended lower, until it fixed round the umbilicus. Having been costive on the first attack, he took several purgatives. His constitution has suffered much injury by drinking spirits to excess. He was directed to take a scruple of *Angustura bark*, in powder, four times a day.

Jan. 3d. He feels himself relieved from the griping round the umbilicus since he has taken the *Angustura bark*; the tenesmus is also abated. The stools are less frequent, and chiefly mucous, without any mixture of blood: his appetite is improving, and he does not feel any return of pain after eating, as before. The use of the bark was continued.

6th. He had only two stools yesterday, and one to-day; has now no complaint from his  
4
bowels;

bowels, and his strength is returning. He was directed to take five grains of calomel in the morning, and to continue the use of the bark a few days longer.

It frequently happens, that persons unaccustomed to this climate, after suffering a severe fit of sickness, are apt to continue in a state of great weakness and irritability for a considerable time, even for several months, without any visible alteration. This most usually happens when the patient has refused to take medicine during the fever, &c. or has not continued its use a sufficient time after the complaint has left him. The consequence is, that towards evening he generally becomes very restless and uneasy; his skin is dry, and hotter than usual; his pulse is not much affected while in a recumbent posture, but on sitting up or using any motion, it is soon accelerated. These symptoms harass the patient during the whole night, but are mostly carried off in the morning by a slight partial sweat upon the head and breast. A slight chill sometimes ushers in these symptoms, and frequently there is pain in the forehead, or crown of the head.

The chief indication in such cases is to strengthen the constitution, for which purpose I have employed a variety of tonic remedies, especially Peruvian bark, Colombo, and Gentian; but I have never found any of these so speedily efficacious as the Angustura bark in restoring strength and appetite.

In a case of Hemisplanchium, which had continued some weeks in a very irritable habit, attended by a train of nervous symptoms, and where blisters had procured only temporary relief, the disease was soon removed by an infusion of Angustura bark, made by infusing an ounce of the powder for two days in a pint of Madeira wine, of which two ounces were taken three times a day.

## CASE VIII.

Simon Elliott, a black man, aged 44, had been for a long time affected with a tertian intermittent. Before he applied to me the paroxysms had ceased, but were succeeded by a large tumour on the left side, which projected considerably below the false ribs, and which was attended with a short dry cough, dyspnoea, and œdematous swellings of the lower extremities,



mities, reaching nearly to the groin. The Squill pill, with calomel, produced no other effect than to render his mouth slightly sore. The *Infus. Nicotian.* was then given to him, and proved a powerful diuretic, but though used very cautiously in small doses, it brought on head-ach, with dimness of sight, and was therefore laid aside. He was next ordered to take *Pulv. Cort. Angust. & Crem. Tart.* aa. ʒss. three or four times a day, which kept his bowels moderately open, and increased the quantity of urine so considerably, that after continuing the use of the medicine for ten days, the swellings entirely disappeared, and by persisting for some time longer in the use of the Angustura bark alone, he completely regained his strength.

I have had but one opportunity of trying the Angustura bark in the form of enema; this was in the case of a gentleman who had been only a few months in the colony, and had suffered greatly from repeated attacks of fever. Having had a relapse while in a state of great debility, his stomach became so weak and irritable, as to reject, almost immediately, every thing he took. The effervescent draughts, joined with opium, produced only a temporary relief, and often failed even in that respect; his  
skin

skin was hot and dry; his pulse from 112 to 120, and small; there was at the same time great restlessness, and his voice was so weak, that he could speak only in a whisper. An enema, consisting of half an ounce of powder of Angustura bark, half a drachm of tincture of opium, and seven ounces of water, was directed for him twice a day; and at bed-time he took an effervescing draught, with *Tinct. Opii*, gutt. xv. By these means the vomiting was checked almost immediately, so that he could retain a little nourishment; his pulse beat stronger, and only 96 in a minute; his skin became moist, and a slight pain over his eyes, which he had before complained of, entirely went off; a small degree of appetite also returned, and he appeared, 'upon the whole, to be recovering his strength. Having a dislike to the enema, it was omitted, after having been used near a week. After continuing nearly a week longer, with the same favourable symptoms, he suddenly fell into a state of coma, which soon carried him off.

I have thus endeavoured to communicate to you whatever has occurred to me respecting the use of the Angustura bark; but several of my papers having been lost or destroyed by the  
French,

French, when they paid us a visit at Sierra Leone, this account is thereby rendered more imperfect than it might have been. Nevertheless, with all its defects, it may tend, in some degree, to prove that the Angustura bark is a valuable acquisition to the *Materia Medica*, and a medicine not unworthy of the praises which have been bestowed upon it.

*Free Town, Sierra Leone,*

*Dec. 24, 1794.*



V. *An Account of a remarkable Affection of the Testes. By Mr. Widdows Golding, Surgeon at Wallingford, in Berkshire, and Member of the Corporation of Surgeons in London. Communicated in a Letter to William Babington, M. D. Physician in London; and by him to Dr. Simmons.*

A SWELLING of the Testes has been mentioned by different writers\* as a symptom of

\* Vide *Russell de Œconomia Naturæ in Morbis acutis et chronicis Glandularum.* 8vo. 1755.—*Rochard, Description*

of the *Cynanche parotidea*, or mumps, as the complaint is vulgarly called; and Sauvages, who has described this disease in his *Nosologia Methodica*\*, as a species of catarrh, to which he has given the name of *Catarrhus Bellinfulanus*, from its having prevailed in Belle Isle in 1757, points out this swelling of the Testes as a characteristic mark by which this disease may be distinguished from other species of catarrh. But I have met with no writer who speaks of a similar affection of the Testes, attended with pyrexia, and occurring without any previous swelling of the parotid or maxillary glands. In the Summer and Autumn, however, of 1793, instances of such

description d'une Maladie particuliere des Glandes, endemique a Belle Isle en mer. *Journal de Medecine*. 8vo. 1757.—Gooch, Cases and Practical Remarks in Surgery. 8vo. 1758.—Cullen, First Lines of the Practice of Physic, Vol. I. 8vo. 1784.—Hamilton, Account of a Distemper, by the common People in England vulgarly called the Mumps. *Transactions of the Royal Society of Edinburgh*, Vol. II. and *London Medical Journal*, Vol. XI.—M. Tissot, in his work, entitled *Avis au Peuple*, (Tom. I. 8vo. Lyon, 1763, § 116) has also spoken of the Mumps under the name of *Oreillons*; but he says nothing of the affection of the Testes.

\* The description given of this complaint by M. Sauvages, is from the account communicated by M. Rochard to the Editor of the *Journal de Medecine*, and referred to in the preceding note.

an affection, where the glands of the face or throat (except only in one of the cases) were not at all concerned, occurred to me, in this neighbourhood, in sufficient number, and with a correspondence of circumstances sufficiently striking to induce me to consider them as the effects of an epidemical state of the air.

As the form of the disease will, I think, best appear from a narrative of the several cases, I shall describe them to you in the order in which they occurred.

## CASE I.

I was desired, in the beginning of June, 1793, to visit a farmer's servant, about eighteen years of age, and of a plethoric habit, who had been ill four days. I found him in bed, labouring under the usual symptoms of fever, and complaining that his left testicle was much swelled, and gave him considerable pain; and that it had continued increasing in size and uneasiness since he was first taken; but what to impute it to he was at a loss to conceive, not having received the least external injury whatever. Upon examination I found it much enlarged  
and



and inflamed, and the spermatic chord increased in size. I inquired if he had observed any discharge previously to its swelling, or had been troubled with any stranguy, not entertaining a doubt myself that the complaint was a *Hernia humoralis*, though the symptoms of fever were apparently more violent than they usually are in such cases. I told him my suspicions of the origin of the complaint; but he convinced me, by the account he gave of himself, and the examination he submitted to, that they were without foundation. Excepting this appearance, he had not the slightest symptom that could be attributed to gonorrhœa or syphilis.

He was let blood; was directed to wear a bag truss; to bathe the scrotum with *Aq. Lithargyr. acet. c.* and to take an emetic (composed of *Hydrargyr. Vitriol. gr. v.* and *Pulv. Ipecacuan. gr. xv.*) in the evening, and an active purgative the day following.

When I saw him again, a few days after, the symptoms of fever were abated; the testicle was less painful, and the inflammation lessened, although the part was not decreased in size; but there was some appearance of corrugation on the scrotum. The spermatic chord remained in the same state as when I first examined it;

but this, with all the other appearances, gradually subsided, and within the space of about eighteen days from the commencement of the complaint, he was perfectly recovered.

## CASE II.

In the beginning of July, 1793, I was desired to visit A. L. a single man, about 23 years of age, by trade a butcher. I found him in bed, labouring under some slight symptoms of fever, though not accompanied with the same degree of inflammatory diathesis, as in Case I. He informed me (to use his own expression) that he had taken cold, and that it was settled in his left testicle. He had then been indisposed three days previously to my seeing him. Upon examining the part affected, I found the testicle much inflamed, enlarged, and very painful, putting on every appearance, externally, of an *Hernia humoralis*; but the spermatic chord was not so much enlarged as in Case I. I had reason to suspect, from the character of this man, that the complaint was syphilitic, although he declared the contrary. I examined him, therefore, with the strictest attention, but there was  
not

not the least appearance of any chancre; he had no discharge or strangury, nor any symptom whatever, that could lead me to consider the case as venereal, excepting the state of the testicle. The mode of treatment adopted in Case I. was pursued in this, and the patient recovered in less time than the former.

### CASE III.

J. H. a single man, about 20 years of age, whose employment was that of a gardener, applied to me about the same time as the last-mentioned patient. He complained of an enlargement of the left testicle, which had been coming on for some days; and upon examination I found it increased in size, and considerably inflamed; it gave him great pain, and was attended with some degree of symptomatic fever. A careful examination, as in the former cases, convinced me there was not the least reason to suppose the complaint venereal; and he soon recovered, by pursuing the means adopted in the former cases; but the affected testicle has gradually become much smaller than the other.

## CASE IV.

J. P. a respectable tradesman, of a corpulent habit, about 53 years of age, desired my attendance Aug. 15, 1793. He requested me to examine his left testicle, which he informed me had given him great pain for two days. I found it much inflamed, and considerably swelled; the chord was also enlarged, and he was affected with some degree of fever.

Now whatever my suspicions, when first applied to, might be of the single men, whose cases I have described, I could not for a moment entertain the same ideas of this patient, who was a married man, of the strictest prudence and sobriety. There was no reason here to suspect any venereal infection; nor was there any external cause to which the complaint could be ascribed. I adopted the same mode of treatment as in the other cases, excepting bleeding, as the patient desired this might, if possible, be omitted, on account of a gouty affection to which he is subject.

From the 15th to the 19th the pain, inflammation, and swelling of the testicle continued to increase, accompanied with thirst, costiveness,

ness, cold chills, high-coloured urine, quick pulse, loss of appetite, and restlessness.

On the 19th four leeches were applied to the scrotum, and, after these, cloths dipped in a lotion composed of Sal. Ammon.  $\mathfrak{z}\text{i}$ . Aq. Lythargyr. acet. c.  $\mathfrak{f}\text{ij}$ . and Aq. Ammon.  $\mathfrak{z}\text{j}$ .

He was directed to take a mild purgative, and afterwards a saline draught, with half a drachm of tincture of antimony, every three hours ; and a similar draught, with the addition of thirty drops of tincture of opium, at bed-time.

Aug. 20th. The inflammation and swelling of the testicle were but little abated, although the pain was not so great. The opiate had procured rest, and the symptomatic fever was lessened. He was desired to continue the use of the lotion and medicines.

22d. The testicle this morning was less inflamed ; the skin of the scrotum appeared corrugated ; the pain was greatly abated ; and the symptomatic heat much decreased. He was now directed to take the bark liberally.

28th. The testicle was much lessened, and free from pain and inflammation.

30th. The testicle appeared to be nearly of its natural size, and the patient was sufficiently recovered to return to his business.



## CASE V.

J. M. a respectable farmer in the neighbourhood of Wallingford, married, of a plethoric habit, and in the 55th year of his age, desired my attendance Sept. 7, 1793. He had been confined a week, with a painful swelling of his right testicle, but could give no cause for the complaint. Upon examination I found it enlarged, and attended with considerable inflammation; the spermatic chord was likewise increased in size, and the patient laboured under the usual symptoms of beginning fever. The same medicines, and applications to the part, were had recourse to as in the last-mentioned case.

Sept. 9th. The testicle appeared much inflamed, and was more painful; the spermatic chord was more enlarged, and there was also an increase of fever. Four leeches were applied to the scrotum.

10th. The testicle was less inflamed, and the skin somewhat corrugated, although the part was but little decreased in size. The symptoms of fever were abated.

12th.

12th. The inflammation, pain, and swelling of the testicle were less, and the scrotum was more corrugated; but the spermatic chord was still much enlarged. The feverish symptoms having subsided, he was directed to take the bark.

15th. He was so far recovered, as to be able to leave his room. The swelling and inflammation of the testicle were much abated, but the chord still remained enlarged.

17th. He was attacked yesterday with a painful swelling of the right side of his face, previously to which he had cold rigors, succeeded by heat. The testicle appeared much less, and free from inflammation.

23d. The swelling of the face had subsided; the testicle and chord were nearly of their natural size, and the patient was in every respect pretty well recovered.

The late Mr. John Hunter, in his justly-celebrated Treatise on the Venereal Disease, (Part. III. chap. 13) relates instances of enlargement and inflammation of the testicle, without any venereal infection; and I myself remember, four years ago, to have seen, in

St. Thomas's Hospital, in London, a married man, fifty years old, who was admitted for the cure of a supposed *Hernia humoralis*, but who was perfectly free from every other appearance of venereal infection. The testicle was considerably enlarged and inflamed, and the complaint soon yielded to the mode of treatment commonly adopted in cases of *Hernia humoralis*.

The instances of this affection, which are more immediately the subject of this paper, having all occurred to me between the months of June and September of the same year, were probably of an epidemic nature, and as such I have thought they may be deemed worthy the attention of medical readers.

Dr. Wall, in a Dissertation on the diseases of the South-Sea Islands, observes, that  
 “ swellings of the glands in the axilla and in-  
 “ guen, and even of the testicle itself, are not  
 “ uncommon attendants of a great variety of  
 “ epidemic fevers, and diseases communicated  
 “ by contagion, though not of a syphilitic  
 “ nature\*.”

\* *Vide* Dissertations on Select Subjects in Chemistry and Medicine; by Martin Wall, M. D. 8vo. Oxford, 1783. p. 158.

In the epidemic affection I have related, the disease was generally ten or twelve days before the part came to its full height of inflammation, and then the complaint gradually subsided, without putting on any appearance of suppuration or tendency to sphacelus. In some of the patients the spermatic chord was larger than in others; and in none of them did it begin to decrease till the inflammation had abated, and the scrotum began to corrugate.

In one of the patients only, as I have already observed, has there been any perceptible wasting of the testicle in consequence of the complaint.

Three of the patients, as we have seen, were young, and single men; the other two were married, and upwards of fifty years of age.

In one of the patients (Case V.) a swelling of the face came on when the affection of the testicle was beginning to subside; but I was inclined to think that this was an accidental circumstance, arising from cold, and of course not connected with the original disease.

*Wallingford,*  
*January 17, 1795.*

VI. *Case of a Man who castrated himself. By Mr. Widdows Golding, Surgeon at Wallingford. Communicated in a Letter to Mr. Thompson Forster, Surgeon of Guy's Hospital, in London; and by him to Dr. Simmons.*

**A**GREEABLY to your request, I now communicate to you the case of the patient who, in a fit of religious enthusiasm, made an incision into his scrotum, and removed both his testicles.

He is an unmarried man, twenty-five years of age, and by trade a bricklayer.

He made the incision, with a pen knife, transversely at the bottom of the scrotum, so that both the testicles came out through the wound. He then divided each spermatic chord about an inch from the body of the testicle. A profuse hæmorrhage succeeded, which he attempted to stop by the application of stinging nettles to the part; but this not answering his expectations, he procured a needle and thread, with which he sewed up the external opening,  
and



and thought all would be well, and that no person would know what had happened.

His friends, however, found out what he had done; and, after about five hours had elapsed, I was applied to, and immediately went to his assistance.

I found the scrotum, perinæum, and left iliac region distended with extravasated blood.

I divided the whole of the tumefied scrotum, and let out above a quart of coagulated blood.

With much difficulty, owing to the retraction of the two chords, I passed a ligature round each of the spermatic arteries; and after applying a poultice of bread and milk to the part, secured by a tight bandage, I directed the abdomen to be fomented, and a suitable regimen to be observed. He took an opiate at night, and the next morning a purgative medicine.

March 17, 1795, (the day after the castration) I found him complaining much of pain. He had made water four times in the night, but had had no evacuation by stool. His pulse was at 120, and full. The penis was much lessened, and the scrotum was contracted. The purgative medicine was repeated.

18th. He had had three stools, and voided  
a suf-

a sufficient quantity of urine. His pulse was at 125, and he complained of pain in the lower part of the abdomen. I observed a considerable swelling of the left spermatic chord, which had come on since the day before.

19th. His pulse was 100. The left spermatic chord was still much enlarged.

20th. His pulse was 100. The swelling of the chord was decreased.

23d. The pulse was 95. The use of the poultice and fomentation was discontinued, and simple dressings were applied to the wound, which granulated daily.

24th. About four ounces of coagulated blood came away last night from the right side of the wound, which had probably lodged about the abdominal ring. He was free from pain.

27th. The ligature of the right spermatic artery was removed, and three days after I removed the other ligature.

From the 21st of March he had taken freely of the bark; but on the 3d of April his health was so well restored, that medicine of every kind was discontinued, and before the middle of April he returned to his usual employment.

*Wallingford,*  
June 13, 1795.

VII. *Cases, and Remarks on the external Application of Charcoal; by Mr. William Simmons, Member of the Corporation of Surgeons of London, and Surgeon to the Manchester Infirmary.*

**I** WAS led to the use of charcoal powder in foul and fetid ulcers from perusing the second edition of Dr. Beddoes's publication on Factitious Airs, and am happy to add my testimony in its favour. From the untoward appearance of three stumps after amputation, which resisted the ordinary means of relief, I willingly gave it trial, and will now relate the result.

### CASE I.

W. Holt, aged 47, was admitted into the Infirmary April 27, for a carious ankle-joint, which was deemed proper for amputation. I accordingly removed it on Friday, the 1st of May, at the usual place above the ankle; and  
brought

brought the integuments into contact so as to unite by the first intention.

An adhesion formed in the lower part of the wound in a few days, but elsewhere the appearance was unfavorable; the symptomatic fever ran high, and the parts covering the fibula ulcerated so as to expose the end of it. The common cataplasm was applied, and saline medicines, with opium, were given according to the indications. The disease, however, went on; the stump assuming a foul and gangrenous appearance, the discharge becoming very fetid, and the inflammation extending rapidly towards the knee. Bark, wine, and sulphuric acid were freely given; and opium, in such doses, and so repeated, as to ease pain; and the fermenting cataplasm was used instead of the common one. By vigorously pursuing these means for several days, the farther progress was checked, and healthy granulations began to appear; yet matter formed between the tibia and fibula in considerable quantity, and of a disagreeable smell. At this period, still continuing the bark and other remedies, the charcoal was substituted for the fermenting poultice. Two drachms of the powder being insufficient, half an ounce was added to enough of the common poultice, to cover

the end of the stump, and as far as the inflammation extended. The discharge was henceforth not offensive; but notwithstanding the remedies were continued in such doses as his stomach would bear, his recovery was so slow as to render it expedient to send him into the country, where a speedy amendment took place, and he was discharged in a few weeks with a good stump.

## CASE II.

Samuel Allwood, aged 23, was admitted at the same time for a caries in the left knee-joint, occasioned by a blow, and of twelve years standing. The degree of disease, and his exhausted state, sanctioned the propriety of early amputation, which was accordingly performed on the same day as the former. At the end of ten days nearly a complete union had taken place, when unfavourable symptoms came on; the pulse became frequent, with general languor, loss of appetite, and a tendency to disunion in the parts. The remedies given in the former case were exhibited in this, and the limb was wrapped up in the common poultice. Nevertheless



theless the recently-connected parts gave way, the whole put on a foul appearance, and produced a very fetid discharge. The common being changed for the fermenting cataplasm, and the medicines continued, in a few days checked the progress of the symptoms, yet the smell of the matter was little mended. The charcoal was therefore applied as in the former case, and with equal benefit. Under this plan of treatment he was recovering, but so slowly, that it was thought advisable to remove him into the country, where medicines were soon no longer necessary, and in a few weeks he was discharged with a tolerably good stump.

The disposition to ulceration in this case was so strong, that a flannel roller, applied only so tight as to prevent the retraction of the integuments, occasioned extensive sloughing, which shewed no disposition to exfoliate, although covered with the carbon, until the removal of the patient into purer air.

### CASE III.

James Nuttall, aged 17, was admitted on the same day for a scrophulous affection of the  
 ankle-

• ankle-joint, which was adjudged, in consultation, proper to be removed. Accordingly I amputated the foot above the ankle on Friday, the 8th of May. In a week union, by the first intention, had nearly taken place, and every thing promised a speedy recovery; but symptoms, similar to those of the former cases, now came on, and increased even to a greater degree of violence. The remedies used in the other cases were given without the smallest benefit; and the pulse became so extremely rapid, that, together with the threatening appearance of the stump, it left but slender hopes of a favourable issue. Half an ounce of extract of bark was exhibited every twenty-four hours instead of the powder, and the other means were persevered in. The parts, however, disunited; and the integuments retracted, so as to expose the end of the tibia and fibula, were exquisitely tender to the touch, and discharged a very fetid ichor. A trial was given to the common and fermenting poultices, and their use superseded, as in the former cases, by that of charcoal, which answered equally well in this instance. The ulceration, however, continued to extend, and the irritation was extreme.

The patient was now removed into a more

airy ward, by which, and a continuance of his medicines, the threatening symptoms somewhat abated till his removal into the country, where also a favourable change soon took place; but the integuments having retracted, so as wholly to uncover the end of the stump, and a variety of other untoward circumstances occurring, he is not yet discharged. On that part of the sound integument, covered by the poultice, a pustulous eruption appeared, which was allowed to spread for several days, but being very sore and painful, it was then dressed with the calamine cerate, which healed it in a few days.

Besides using the remedies above mentioned, the patients were all allowed to eat freely of oranges.

For the introduction of fixed air into practice we are indebted to Dr. Macbride, to whom it was suggested by an observation of Sir John Pringle. It has since been employed in diseases of a putrescent tendency, in calculous cases, and in foul and ill-conditioned ulcers. The carrot poultice, the fermenting cataplasm, and streams of fixed air, extricated from substances containing it, have been used in cases where an ulcer has been foul, or the parts in a state of gangrene. To apply the latter is often-

I

times

times impracticable; the fermenting cataplasm, after lying on the part some hours, becomes more offensive than the smell it was intended to cover; and carrots, which are not always to be met with, contain a resinous matter that may in some instances be improper. Charcoal, a substance containing carbonic acid gas, is free from these objections. In the cases lately published, it has been said to possess the power of cleansing and healing ulcers, as well as correcting the discharge; the present cases warrant no such conclusion. Holt and Allwood were better before it was used, and during the whole time powerful internal means were exhibited. The sloughy state of Allwood's limb, covered by the carbon, led me to attend particularly to that circumstance, and to render that opinion at least doubtful. The case of Nuttall, however, is conclusive. Every means suggested to check the rapid progress of his disorder was ineffectual for some time, and the ulceration extended, although the limb was covered with a thick poultice of the carbon. But what puts it beyond a doubt is the pustulous appearance which came upon and spread on the sound part of the limb covered by it, which was healed in a few days by the ceratum lapidis calaminaris.

The change wrought by any substance on dead animal matter, is no proof of its operation on the living fibre. From the above cases it appears that charcoal corrects the fetid smell of the matter issuing from a foul ulcer, but has no power in promoting cicatrization. The cases related in the work above alluded to, are not quite satisfactory; for whilst this substance was employed, remedies generally efficacious were administered, except in two instances which are so shortly reported as to prove nothing.

From its success in foul ulcers I was induced to try it in ulcers attended with caries; and in these it has fully answered my expectations. It never fails to correct the stench, a circumstance of great comfort to the patient himself and all around him. Besides the unavoidable contamination of the air in apartments where many invalids are put together, the effluvia arising from foul and carious ulcers must have a pernicious effect, by adding to the impurity, and also tending to produce inbred diseases. In some ulcers, where the discharge is not only fetid, but so acrimonious as to excoriate the neighbouring parts, I have found that it obtunds the acrimony; and as an ulcerated is the best absorbing surface, may it not produce a  
farther



farther benefit, by thus preventing a pernicious substance from being carried into the system?

I am aware that three instances hardly corroborate evidence in favour of any remedy; but having since tried it in a great variety of cases, I relate them as being the first, and which were very particularly attended to. Ample subsequent experience has convinced me that I have fairly appreciated its merits; to detail the many cases in which I have tried it would be unprofitable labour, as their result corresponds with these.

It is hardly necessary to observe that the quantity of powder used should be according to the extent of surface to be covered, and that it should be well washed and dried to free it from salts or other foreign matter.

Changing the poultice once a day is sufficient.

In the army and in military hospitals, where, from the number of surgical patients and other causes, attention to their cases cannot be so regular, as in common practice, I apprehend it will be found a very valuable acquisition.

*Manchester,*

*November 10, 1795.*

N. B. The Manchester Infirmary is situated in an airy and elevated part of the town; the

wards and bedding are kept very clean. It is manifest, however, that the air is less pure than in the country: the liberality of the trustees, therefore, keeping pace with the wants of the suffering objects, enables the physicians and surgeons to provide lodgings in the country, to secure or expedite the recovery of those whose cases may require removal.

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VIII. *Case of Pins extracted from the Breast of a Woman, after remaining there sixty Years. By Mr. Henry Fryer, Surgeon, at Stamford, in Lincolnshire. Communicated in a Letter to Dr. Simmons, by John Clarke, M.D. Physician in London.*

*To Dr. SIMMONS.*

DEAR SIR,

HAVING been favoured by Mr. Fryer, a respectable surgeon at Stamford, with the following case, I am induced to communicate it to you as an uncommon instance of the great length of time extraneous substances may remain in the body without producing any disturbance.

There

There are few circumstances which admit of greater variety than the effects of extraneous substances introduced into the body. Instances must be familiar to every practical man, in which very considerable inflammation has been produced by them, and kept up till they have either been extracted by art, or evacuated by some natural process; whilst, on the contrary, there are other cases in which they have remained for many years without any inconvenience to the patient.

Of this the following case is a remarkable example, more especially as the nature and form of the substances so introduced were particularly calculated to stimulate.

I am, &c.

JOHN CLARKE.

*New Burlington Street,*

*March 1, 1796.*

## CASE.

In March last I was desired to attend Mrs. M. The person who came for me informed me, that the patient, a few weeks before, had fallen

down stairs upon her face, by which fall she had hurt her breast, and then first acknowledged that she had, many years before, thrust several pins into her breasts. Upon examination I found I could very readily shake about a great number of pins, (which seemed of different sizes, as well as shapes) not only in each breast, but also in the skin upon the scrobiculus cordis.

On farther inquiry I learned, that about sixty years ago, when between fifteen and sixteen years old, she had been in some degree deranged, and had at that time forced these pins into the skin, but had never felt any inconvenience from them till now, when she had fallen down; and, I suppose, some of the pins being pressed hard against the skin, as they lay between the skin and the ribs, with scarcely any adipose substance intervening, had occasioned the pain. She was perfectly rational at this time, and gave me the above account herself.

As she pointed readily to those pins which gave her pain, I made a small opening with a lancet, and took out three, one crooked, the others straight; they were turned very black, and I had some difficulty in detaching them. I saw her again in a few days, when the wound  
was

was nearly healed, but she complained then of some others; I therefore made another small opening, and removed two more pins, after which she would not consent to have any more taken away, as she said that she then felt no pain from them.

The number of pins still remaining I can by no means guess at; but it is so considerable, that I can feel them strike against each other upon taking hold of either of her breasts, particularly as her skin hangs loosely from her, she being now very thin; and also upon laying my hand upon the skin between her breasts, I can feel them in lumps, lying in every direction.

By way of postscript to this extraordinary proof of the little trouble that extraneous bodies will sometimes occasion, I will add the following :

In the year 1787 I took out of a man's leg a thorn, nearly two inches long, which had lain there twenty-two years, without giving him any pain; and after it was taken out the wound healed in two or three days without any trouble. The thorn lay lengthways on the outer  
edge



edge of the gastrocnemius, near the upper part of that muscle.

*Stamford,*  
*October 6, 1795.*

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*IX. Description of a new Key Instrument for the Extraction of Teeth. Communicated in a Letter to Dr. Simmons, by Mr. J. Savigny, Surgical-Instrument Maker in London.*

*To Dr. SIMMONS.*

SIR,

I BEG leave to present you an engraving of a Key Instrument, for extracting teeth, which I have lately invented, and in the construction of which I have endeavoured, as much as possible, to combine simplicity with utility. The complaints I have been continually accustomed to hear of the pain and danger attending this operation, have long excited my attention, and have led me to conclude that somewhat essentially

essentially deficient must have attended the construction of the different instruments hitherto employed to effect it.

On comparing and reflecting on the various forms which the key instrument has received, I have ever found their principal defect to arise from the depth of the bolster, which, even in the smallest, describes in its action so large a circle (the stem or shank of the instrument being considered as its centre) as to occasion unavoidable inconvenience; and in the larger or deeper ones certain danger of fracturing the alveolar process, and of being followed by consequences always painful, and frequently dangerous.

The violent effects of so powerful a fulcrum have usually been increased by a curve or neck at the inferior extremity of the instrument, for the purpose of retaining its action in a right line with its handle, when employed in the extraction of the molares in the internal direction; an alteration which, although it effected its intended purpose, still augmented the inconveniences I have stated to so great a degree, that I believe I may safely assert, it has uniformly produced, more or less, the pernicious consequences mentioned above.

For an essential improvement in this part of  
the

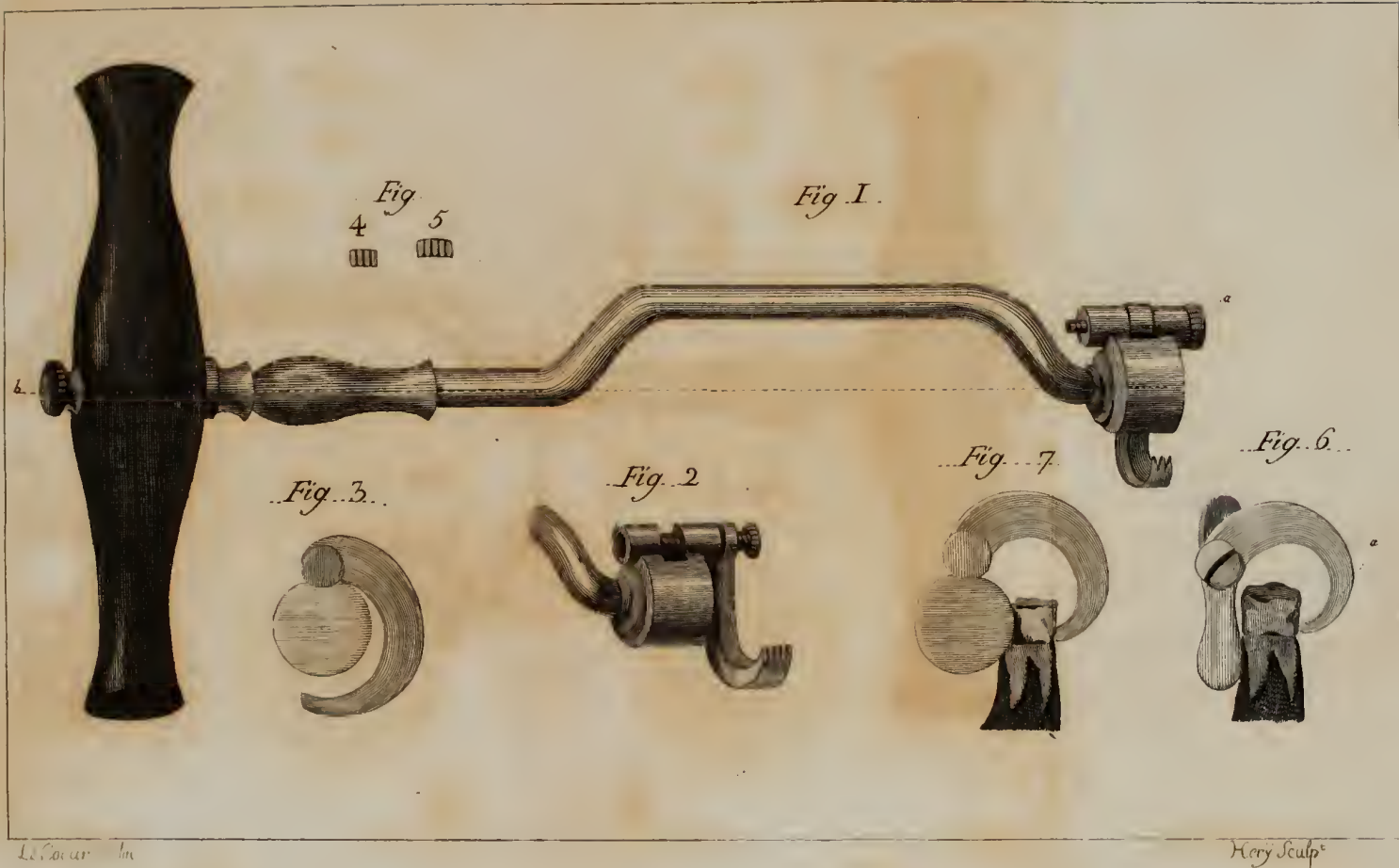
the construction of my instrument, I acknowledge myself indebted to a very intelligent and ingenious paper by Mr. Robert Clarke, of Sunderland, inserted in your last volume \*. I found it admirably calculated to assist the mechanical intention of the circular bolster I have adopted, the action of which being confined to a revolution upon its own axis, gives sufficient power for the extraction of the tooth, in nearly a perpendicular direction. A bolster of this shape may be applied without violence to the process, and by presenting a regular obtuse surface to the gum, lessens the danger of bruising or laceration; while it affords, at the same time, a resisting point to the claw, in whatever position it may be engaged.

The construction of this instrument will be best understood from the annexed engraving †, in which,

Fig. 1. represents the instrument of its proper size and figure; the dotted line is intended to show, that notwithstanding its advantageous curve, its action is perfectly central, and in a right line with its handle.

\* Vol. VI. p. 120.

† See Plate I.







*a* refers to a screw with a milled head, retaining the claw :

*b* to another screw with a milled head, securing the handle, which, by this means, is easily wholly removed, to render the instrument more portable, or its position changed, (horizontally or vertically) as may be occasionally required.

Fig. 2. shows the bolster of the instrument, with the claw attached to the projecting extremity, for more conveniently fixing it on the *dentes sapientiæ*.

Fig. 3. exhibits a front view of the bolster, to show its form and circumference.

Fig. 4. and 5. show the dimensions of the points of the claws, the only difference of size requisite in this instrument.

Fig. 6. represents the bolster and curved neck of the common instrument applied to a tooth. The dotted line, marked *a*, pointing out the direction of its action, renders any comment upon the consequences unnecessary.

Fig. 7. shows the bolster and claw of the improved instrument also applied to a tooth, by which the comparative difference of the powers of the two instruments may be easily ascertained.

The test of actual experiment having confirmed the advantages of this instrument, I submit this account of it to your consideration, and if thought worthy a place in your truly important and useful publication, its insertion will be deemed a favour conferred on,

SIR,

Your most obedient

And very humble servant,

JOHN SAVIGNY.

*King Street, Covent Garden,  
May 23, 1796.*

X. *Some Account of the Effects of the Vapour of Vitriolic Æther in Cases of Phthisis Pulmonalis. Communicated in a Letter to Dr. Simmons, F. R. S. by Richard Pearson, M. D. Member of the Royal College of Physicians, London; and Physician to the General Hospital near Birmingham.*

HAVING for the last two years prescribed the vapour of vitriolic æther to patients labouring under phthisis pulmonalis, and having, both in hospital and private practice, experienced the best effects from its use in this frequent and formidable disease, I am preparing to lay before the public a report of the cases in which it has been given, accompanied with remarks on some other remedies that may be employed with advantage in the cure of consumptions. Being desirous, in the recommendation of a new medicine, to have my own evidence supported by the concurrent testimonies of other practitioners, I take the liberty of calling your attention to this subject, and of submit-

submitting to your notice my method of using this application, which is simply this: I direct the patient to pour one or two tea spoonfuls of pure vitriolic æther (or of vitriolic æther impregnated with cicuta, in the manner hereafter described) into a tea cup or wine glass, and afterwards to hold the same up to the mouth and draw in the vapour that arises from it, with the breath, until all the æther is evaporated. This is repeated three, four, or five times in the course of a day, for a month or six weeks, more or less, according to circumstances\*. The *first effects* of this application are

\* The loss of a part of the vapour, which is unavoidable in this mode of applying it, may be prevented, as a medical friend has suggested, by setting the tea cup containing the æther in a small basin, and inverting a funnel over it. By applying the mouth to the tube of the funnel and making an inspiration, the patient draws in all the vapour along with the atmospheric air, which enters at the bottom of the funnel. In winter, the evaporation may be promoted, by setting the tea cup in hot water; in which case the funnel is to be inverted, *not* into the basin containing the water, but over both tea cup and basin, so as to rest immediately upon a table, tray, or plate, having a bit of doubled paper, or a quill, put under it, to allow the external air to enter more freely.

Children

are an agreeable sensation of coolness in the chest, an abatement of the dyspnœa and cough, and after ten minutes or a quarter of an hour, easier expectoration. The *ultimate effects* (provided other proper measures be not neglected, for this is not to supersede the use of other medicines, but to be employed in conjunction with them) are, a removal of the local inflammation, a cleansing and healing of the ulcerated lungs, and a suppression of the hectic fever. To assert that all these beneficial consequences will flow from its application in every species and degree of phthisis pulmonalis, would be adopting the language of quacks, and insulting the understanding of every one experienced in the profession; but to say that some of these good effects are likely to result from its use in most instances, and most of them in a great number of instances, is only asserting what an experience of two years in a situa-

Children and even infants may be made to inhale this vapour, by wetting a handkerchief with æther, and holding it near the nose and mouth. It must be confessed, that this is attended with great waste; but in urgent cases of whooping cough and croup, in which it promises to be of use, this consideration can have little weight.



tion where the opportunities of making trial of it have been very frequent, has fully confirmed.

The salutary operation of æther applied to the lungs in the form of vapour, I have found to be greatly promoted by several volatile substances that are soluble in it; but by none more so than by cicuta. By macerating a sufficient quantity of the dried leaves of this plant in æther for the space of three or four days, or at most a week, and occasionally shaking them together, a very saturated tincture is obtained, which may be inhaled in the same manner, and in the same doses as the pure æther. My proportions are a scruple, or half a drachm, of the powdered leaves to every ounce of æther. The narcotic particles of the cicuta, conveyed in this manner along with the æther-vapour to the diseased lungs, act as a topical application, with the best effect; hence, æther thus impregnated succeeds, in most instances, better than when it is employed alone. The only unpleasant circumstance attending the inhalation of this æthereal tincture of cicuta, is a slight degree of sickness and giddiness, which, however, soon go off.

It cannot be expected that I should here point out every symptom, or set of symptoms,  
which

which indicate or forbid the use of this application. I shall only remark, that it appears to be best suited to the florid, or what is commonly termed the scrophulous consumption. Where the pulmonic affection is complicated with mesenteric obstruction, or diseases of the other viscera, or a dropical condition, it affords but transitory relief; and in the very last stage of the disorder, the proper time for using it is past.

Should you be induced, Sir, by this address, to make trial of the vapour of vitriolic æther impregnated with cicuta, in phthifical cases, I shall be glad to be favoured with your remarks and observations upon it, whether in its favour or not.

N. B. In catarrhs, the æther-vapour, without the cicuta, succeeds very well. In these cases it is seldom necessary to continue the inhalation more than three or four days, or a week at farthest.

*Birmingham,*  
*July 1. 1796.*

XI. *Account of two Instances of uncommon Formation in the Viscera of the Human Body.* By Mr. John Abernethy, *Assistant Surgeon to St. Bartholomew's Hospital.* Vide *Philosophical Transactions of the Royal Society of London, for the Year 1793.* Part I. 4to. London, 1793.

THE peculiarities of the first of the two cases described in this paper, consist in an uncommon transposition of the heart, and distribution of the blood vessels; together with a very remarkable, and, perhaps, singular formation of the liver. The body, which contained these deviations from the usual structure, was brought to Mr. Abernethy for dissection; with its history whilst alive he is, he observes, unacquainted. The subject, we are told, was a female infant, which measured two feet in length; the umbilicus was firmly cicatrized, and the umbilical vein closed: from these circumstances our author concludes that it was about ten months old. The muscles of the child, which were large and firm, and covered

vered by a considerable quantity of healthy fat, together with the general appearance of the body, strongly implied, it seems, that the child had, when living, possessed much vigour of constitution.

Mr. Abernethy first relates those varieties of the sanguiferous system which were found on the thoracic side of the diaphragm; and afterwards describes those which were discovered in the abdomen: this naturally leads him to the account of the uncommon state of the liver.

The situation of the heart, he observes, was reversed; the basis of that organ being placed a little to the left of the sternum, whilst its apex extended considerably to the right, and pointed against the space between the sixth and seventh ribs. The cavities usually called the right auricle and ventricle were consequently inclined to the left side of the body; therefore, to avoid confusion in the description, our author follows Winslow, in terming them anterior; whilst those cavities usually called left, he terms posterior. The inferior vena cava, we are told, passed, as usual, through a tendinous ring in the right side of the centre of the diaphragm; it afterwards pursued the course of the vena azygos, the place of which it supplied: after

having united with the superior cava, the conjoined veins passed beneath the basis of the heart, to expand into the anterior auricle. The veins returning the blood from the liver united into one trunk, which passed through a tendinous aperture in the left of the centre of the diaphragm, and terminated immediately also in the anterior auricle.

The distribution of blood to the lungs, and the return of it from those bodies, were accomplished after the usual manner.

The aorta, after it had emerged from the posterior ventricle of the heart, extended its arch from the left to the right side, but afterwards pursued its ordinary course along the bodies of the dorsal vertebræ.

From the curvature of the aorta there first arose the common arterial trunk, which, in this subject, divided into the left carotid and subclavian arteries; whilst the right carotid, and subclavian, proceeded from the aorta by distinct trunks.

The inferior aorta gave off the cæliac, which, as usual, divided into three branches; but that artery which was distributed to the liver appeared, it seems, larger than common; and exceeded, by more than one-third, the size of the  
splenic



splenic artery of this subject. This was the only vessel, our author observes, which supplied the liver with blood, for the purpose either of nutrition or secretion.

The vena portarum was formed in the usual manner, but terminated in the inferior cava, nearly on a line with the renal veins. The umbilical vein ended in the hepatic vein.

The liver was of the ordinary size, but had not the usual inclination to the right side of the body; it was situated in the middle of the upper part of the abdomen, and nearly an equal portion of the gland extended into either hypochondrium.

The gall bladder lay collapsed in its usual situation; it was of a natural structure, but rather smaller than common; and was found to contain a tea spoonful of a fluid which, in its colour and other properties, resembled the bile of children.

The intestines did not contain much alimentary or fœcal matter; this was, however, we are told, as usual, deeply tinged with bile.

The spleen consisted of seven separate portions, to each of which a branch of the splenic artery was distributed. The other viscera were

found, and of their usual structure and appearance.

Mr. Abernethy could discover no cause to which the child's death could be assigned. He observed that the tongue was incrusted with a dark coloured mucus, which indicated the existence of fever previously to the infant's death.

When an anatomist, he observes, contemplates the performance of biliary secretion by a vein, a circumstance so contrary to the general economy of the body, he naturally concludes, that bile cannot be prepared unless from venal blood; and he also infers, that the equal and undisturbed current of blood in the veins is favourable to the secretion; but the circumstances of the present case, in which bile was secreted by an artery, prove, he thinks, the fallacy of this reasoning. He regrets that only so small a quantity of this bile could be collected from the gall bladder; as, surely, it was, he observes, very desirable to ascertain more accurately how far the qualities of this curiously-prepared fluid resembled common bile.

That the fluid secreted by the liver was not, in this case, deficient in quantity, appears to him sufficiently evident. If the gall bladder had not suffered occasional repletion, he thinks

it

it would have been found in a state of greater contraction. Some bile, he observes, had escaped from the divided gall-ducts, in removing the stomach and duodenum, before the uncommon termination of the vena portarum was discovered; and a considerable quantity of this fluid, he adds, would be required to give so deep a tint, as in this case was visible, to the alimentary matter.

He supposes, therefore, that the empty state of the gall bladder was the effect of accident, and not of deficient secretion by the liver. The bulk and well-nourished state of the body do, he thinks, demonstrate that there was no defect in the functions of the chylopoetic organs.

But it will probably be inquired, from what cause the death of the child originated. It may, our author observes, be suspected that the mal-formation of the liver contributed to its decease; and particularly as no derangement of any vital organ could be discovered. Yet if it be considered how frequently children die from nervous irritation, or fever, the probability of this suspicion is, he thinks, diminished. The circumstances of the case, he observes, may impress others with contrary sentiments; but he himself will remain satisfied with

with having faithfully described the appearances of the body, and having offered those remarks which he thinks deducible from them.

The appearances described in this case are represented in two plates, for which we must refer our readers to the work itself.

The peculiarity of the next case, related by our author, consists in an uncommon formation of the alimentary canal. The subject of it was a boy, whose body was brought to him for dissection; it measured four feet three inches in length; but was well formed, and had moderately large limbs, but flabby, as if wasted by recent disease.

Upon opening the abdomen, which was enormously swollen, there appeared a more than ordinary extent of large intestines, in a state of great distention.

The diameter of the canal measured about three inches, and its dimensions were nearly equal in every part.

The matter, we are told, with which it was turgid was of a greyish colour, of a pulpy consistence, having little fœtor, and quite unlike the usual fœcal contents of the large intestines.

The length of the colon, Mr. Abernethy  
observes,

observes, was uncommon; for after having, as usual, ascended to the right hypochondrium, it was reflected downwards, even into the pelvis; it then, it seems, reascended to the left hypochondrium, and afterwards pursued its usual course.

After turning aside this large volume of intestine, to examine the other parts of the alimentary tube, our author was surprised to discover that the subject contained scarcely any small intestines. These viscera, with the stomach, lay, he observes, in a perfectly collapsed state; and their texture was so extremely tender, that they were torn even by a gentle examination. The duodenum, jejunum, and ileum, when detached from the body, and extended, measured, it seems, only two feet in length, whilst the extent of the large intestines exceeded four feet.

The utmost length of the intestinal tube, in this subject, was little more than six feet, whereas it should, it is observed, have been about twenty-seven feet, had it borne the ordinary proportion to the length of the body.

Mr. Abernethy distended and dried this curious alimentary canal, and still has it in preservation.

As



As the small intestines measured only two feet in length, this extent, he observes, was doubtless insufficient for the preparation and absorption of chyle; these processes must therefore, he thinks, have been, in a great degree, performed by the large intestines.

The form and stature of the boy, our author remarks, show that nutrition was not scantily supplied; he died, he thinks, evidently from a want of intestinal evacuation. Whether the unusual structure of the canal contributed to the production of disease, cannot, he observes, be readily determined; it appears, however, he adds, very probable that uncommonly formed parts, although capable of supporting life, may be less adapted to sustain the derangement of functions consequent to disease.

XII. *Description of an extraordinary Production of Human Generation; with Observations.* By John Clarke, M. D. Vide *Philosophical Transactions of the Royal Society of London for the Year 1793.* Part II. 4to. London, 1793.

THE expulsion of the monstrous production here described succeeded the birth of a perfect and healthy child, in the General Lying-in Hospital in London. It was inclosed in a distinct bag of membranes, composed of decidua, chorion, and amnios, and had a placenta belonging to it, the side of which was attached to the placenta of the perfect child.

The substance contained in the membranes was covered with the common integuments, and of an oval figure, about four inches in length, and three in breadth; and near the centre of it there was a small funis, about an inch and a half in length, by means of which it was connected to the placenta.

On its surface were seen two imperfect resemblances of feet, on one of which were one  
1 large

large and three smaller toes, and on the other one large and two smaller toes.

Between the two feet was situated a small and rounded projection, into which a small passage led, capable of containing a bristle, but it soon terminated in a *cul de sac*. Close to the funis there was another small and thin projection, about a third of an inch in length, which looked like a finger, and was found to contain bony matter, and joints.

There was no appearance of head or neck; of clavicle, scapula, or upper extremities; of legs or thighs; or of organs of generation. The only external marks of resemblance it had to a human foetus consisted of its covering, and the attempt at a formation of two feet and a finger.

Before its internal structure was examined, the navel string of the perfect foetus was injected; and from this, we are told, the injection very readily passed through both placentæ, and even into the substance of this monstrous production, as appeared by the redness of the skin.

On dissecting off the skin it appeared, that of the two imperfect feet, the upper one was connected to the internal parts only by cellular  
membrane :

membrane ; but that the lower one was articulated to the inferior parts of a tibia and a fibula.

Internally this production was found to consist of an homogeneous fleshy texture, (very vascular, but without any regular or distinct arrangement of muscular fibres) surrounding an os innominatum, os femoris, tibia, and fibula. Of these bones the two first, it seems, were perfect, and of the size usually met with in a foetus at the full period of uterine gestation ; but the tibia and fibula were much shorter than in their natural proportion to the thigh bone.

At the upper part, and towards the inside of the os innominatum, was found a little portion of small intestines, loosely connected, by their mesentery, to the posterior edge of that bone, where it is commonly united to the os sacrum. These intestines, it seems, had a covering of peritonæum, and were very minutely injected.

Our author's next object was to trace the vessels of the funis. There appeared to be only two, viz. an artery, and a vein ; and these passed on towards the inner surface of the os innominatum. As they approached this bone, they gave off some branches to the surrounding parts, which quickly became too small to be traced.

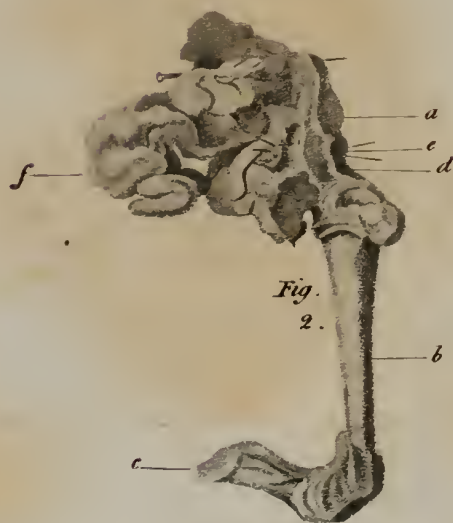
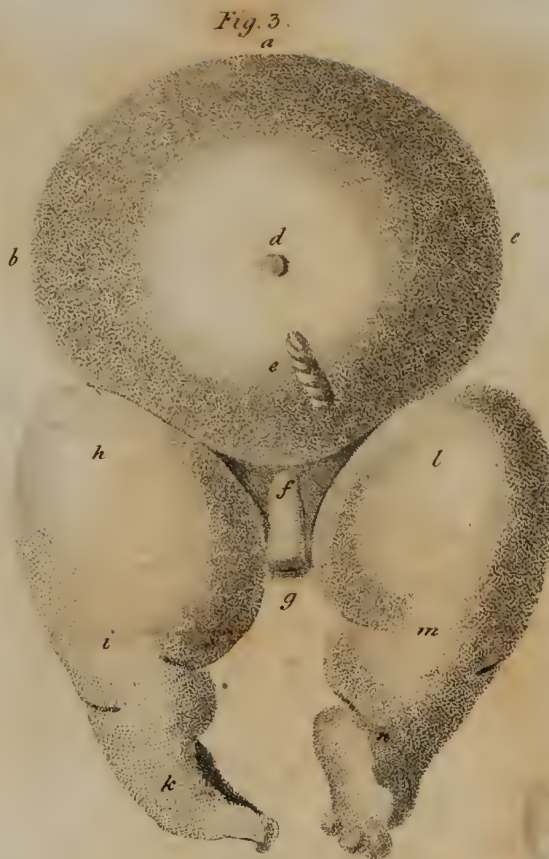
traced. The trunks then passed backward, towards that part where the articulation with the os sacrum is generally found; at which place they went to the other side of the bone, where they distributed a great number of small branches, and were at length lost in the surrounding parts.

This, we are told, was the whole of the internal construction of this very extraordinary monster. There was not the smallest appearance of vertebræ, or ribs. There was neither brain, spinal marrow, nor nerves. It had no heart, nor lungs. It contained none of the viscera subservient to digestion, excepting the intestines already mentioned; nor any glandular substance whatsoever.

To his description of this singular production, which is illustrated by two very accurate engravings\*, Dr. Clarke has added several judicious

\* In the annexed plate these are copied on a reduced scale of one half the diameter of the original engravings. See Fig. 1. and 2. of Plate II. Fig. 1. exhibits a view of the external appearances: in this figure *a* refers to an imperfect formation of a foot, with four toes upon it; *b* to an imperfect formation of another foot, connected to the tibia and fibula, and having three toes upon it; *c* to the projection into which







dicious and interesting observations, which the circumstances of the case seemed to him naturally to suggest.

The mere description of any monster, as he very properly remarks, is of little utility, unless it tends to explain some actions of the animal economy, before imperfectly, or not at all understood. It is on this account, he observes, that very few additions have been made to the stock of our knowledge, from considering those monsters in which there are either supernumerary or confused parts; because, if we cannot

which a duct led, terminating in a blind pouch; *d* to the funis umbilicalis; and *e* to an imperfect formation of a finger. In Fig. 2. which exhibits an internal view of the parts, as they appeared after clearing away the fleshy matter from the bones; *a* refers to the os innominatum; *b* to the os femoris; *c* to the tibia and fibula, to which the lower foot was connected; *d* to the funis umbilicalis; *e* to two bristles passing, in the vessels of the funis, to the outside of the os innominatum; and *f* to a portion of small intestines, terminating in a *cul de sac* at each extremity.

In this figure, through a mistake of the engraver who copied it, the line of reference at *d* is made to terminate at the outside of the os innominatum, instead of extending (as at *d*, fig. 1.) to the funis umbilicalis; the situation of which, however, is sufficiently pointed out by the inner ends of the two bristles referred to at *e*.

distinctly perceive the use, or necessity of parts, in their natural state, we are not likely to advance in information by the examination of those varieties of structure, where difficulties are only multiplied by the greater complication, or aggravated by the confusion of parts. The only useful inference in natural history which, he thinks, can be drawn from monsters of the last kind is, that nature can deviate from the usual arrangement of parts, without any material inconvenience; and therefore, that the existence of parts so as to be capable of being applied to the purpose for which they are intended, in the perfect state of the system, rather than any precise order of them, is required for carrying on the functions of an animal body.

Monsters, however, in the structure of which considerable parts are wanting, seem, to him, peculiarly likely to assist us in the prosecution of physiological researches. For if we were never to see an animal except in its perfect state, we could, he observes, form no just idea of the comparative necessity of the different parts; and if we were to attend only to the complete structure which obtains in the more perfect animals, we might be led falsely to  
conclude,

conclude, that the usual connexion of parts, which we find in them, is essential to the structure and composition of animal matter. Of these parts, the brain and nerves, the stomach and digestive organs, the heart and lungs, appear to be of such importance, that one might be induced to imagine that the functions of life could not be carried on without them : but in tracing the works of nature downwards, says our author, we shall at length find animals gradually becoming more and more simple in their construction. The brain and nervous system are altogether wanting in some, and there are others which have neither heart nor lungs ; yet they continue to exist, and are capable of performing the most important functions of animals : and thus, he adds, the formation of one animal serves to throw light upon the economy of others.

Dr. Clarke is aware that this great simplicity of structure is found chiefly in animals, the texture of whose bodies is nearly homogeneous ; not consisting, as in more perfect animals, of parts so different from each other, as skin, intestines, &c. are from bone : and that it may therefore still be supposed, that all the compli-



cated mechanism, found in the more perfect animals, is essential to the construction of such heterogeneous substances as those of which they consist.

To investigate this matter, he thinks, we must have recourse to those monsters in which there is a deficiency of parts.

There is, he observes, a very material difference between the nature of the life of the more perfect animals, during their foetal existence, and after their birth. In the latter state, the brain and nerves appear to be so essential, that any very considerable defect in them is incompatible with the well-being of the animal; but in the foetal state, considerable deviations from the ordinary arrangement of parts, and such as cannot be endured after birth, are supported without any inconvenience.

In proof of this our author remarks, that the brain has been frequently found very incompletely formed, and sometimes not at all, yet still there have been nerves; and that in other cases, where the brain has been perfect, the spinal marrow has been deficient in a great part of its extent, and sometimes throughout.

Both these circumstances, he thinks, are  
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sufficient

sufficient to prove, that, at any rate, that intimate connexion of the brain and nervous system, which takes place after birth, is not necessary for the formation of a body in other respects perfect. But still, he adds, it would remain doubtful, whether any regular structure could be formed, without any vestige of either brain or nerves; and therefore without a possibility of their influence, in any manner, toward such structure.

The monster, which is the subject of this paper, is, he observes, so extremely simple, in this respect, that it cannot be exceeded by the most simple animal known.

To those who may be disposed to object that there might be brain, or nervous fibres, in this monster, but that they might, in the dissection, be destroyed, our author replies, by observing, that the parts were examined too carefully to warrant such a suspicion; and that as there were no bones representing either the cranium, or spine, or os sacrum, it is not probable that their contents should exist in any other situation.

He is aware that another objection may perhaps be taken from the anastomosis of the vessels of the monster, with those of the perfect foetus, and that the nervous influence may be

supposed to have been transmitted, in this way, along the vessels; but he contends, there is very good reason for believing that the vessels of the placenta have no nerves; and that even if they had, it is still very unlikely that, merely by such anastomosis, any nervous influence could be conveyed.

Dr. Clarke has thought it right to answer another objection which may be made, viz. that nervous matter may be co-extended, or co-existent with all other animal matter, and that, of course, it is of no consequence whether there be any sensorium, or reservoir of impressions, &c. or not; because the stimulus, which produces action, must reside in parts, as well as the other substance of which they are composed: but although this may possibly be true, we have, he contends, no evidence of the fact sufficiently satisfactory to carry conviction along with it. On the contrary, he thinks there is good reason for believing that nervous influence is conveyed from the brain downwards; and if we are right in this conjecture, which is warranted by the experiment of tying, or cutting nerves, then, he observes, the existence of the nervous fibre, like that of a string of a musical instrument, would be inactive, unless it received

ceived an impression, which, with regard to the nerves, should come from the brain.

The whole, then, of the actions of this monster must, he thinks, have been those of the vascular system entirely; and these, he observes, seem to have been capable of forming bone, skin, cellular substance, ligament, cartilage, intestines, &c.

From the defect of heart in this monster, Dr. Clarke argues, that the energy of the arteries was equal to carrying on the circulation, not only in its own body, but also through its own placenta; and from the deficiency of its nerves, that the use of these is probably very small, if any, to the fœtus.

He mentions it as an opinion entertained by a very acute physiologist, the late Mr. John Hunter, that in all cases a fœtus is a very simple animal; as to its internal actions; and the circumstances attending this monster, he thinks, fully confirm such an idea.

Dr. Clarke observes, that in the formation of a fœtus the usual objects of nature seem to be that it should grow, and that it should be fitted with parts which, though of no use to it then, are essential afterwards. We know, says he, that the lungs are of this kind, and it is very

likely, he adds, that the brain and nerves are so too. That there is a very material difference between the internal functions of a foetus in the womb, and those of an infant after birth, seems, he thinks, very presumable; not only from our finding that it can carry on life without parts which are of the greatest moment afterwards; but also from its possessing parts which after birth go into decay, or disappear, as the thymus gland, &c.

The common uses of the nervous powers are, to convey impressions from without, and volition from within; but a foetus in the uterus, observes our author, is exposed to no external impressions, and is most probably incapable of volition, since it is not conformable to the general wisdom of nature to give that which, in such a situation, must be useless. He seems, therefore, inclined to think that the formation and growth of a foetal body depend entirely on the actions of its vascular apparatus.



XIII. *On the Conversion of Animal Muscle into a Substance much resembling Spermaceti.* By George Smith Gibbes, B. A. of Magdalen College, Oxford. Vide *Philosophical Transactions of the Royal Society of London, for the Year 1794*, Part. II. *And for the Year 1795*, Part II. 4to. London, 1794—5.

**I**N a former volume\* of this work, some account was given of the discoveries lately made at Paris, relative to the conversion of certain parts of the human body, under particular circumstances, after death, into a substance resembling spermaceti. M. Thourer, to whom we owe the first description of this phenomenon, seemed disposed to ascribe it to the extrication of aeriform fluids from the dead body during putrefaction, and to the reaction of those fluids on the body itself; but the experiments related by Mr. Gibbes in the two very ingenious papers on this subject now be-

\* Vol. I. page 186.

fore us, serve to show that the putrefactive ferment is not at all necessary in the formation of the substance in question.

It is a matter of great curiosity, our author remarks, to observe, after any fact has been well ascertained, how many things might have led to a much earlier investigation of it; particularly so, had the writings of many great men been equally examined, with those observations which, though apparently very trifling, have often excited general attention. Of this we have a very striking example in the conversion of animal muscle into a fatty matter, a fact of which Mr. Gibbes has discovered traces in the works of two celebrated philosophers of our own country, Sir Thomas Brown, and Lord Bacon. The former of these, in his very learned and curious treatise, entitled *Hydriotaphia*, assures us that he has found a soap-like substance in an hydropical body. His words are as follow, viz. “ In an hydropical body, “ ten years buried in a church-yard, we met “ with a fat concretion, where the nitre of the “ earth, and the salt and lixivious liquor of “ the body, had coagulated large lumps of fat “ into the consistence of the hardest Castile “ soap; whereof part remaineth with us:” and

Lord

Lord Bacon, in his work entitled *Sylva Sylvarum*, expressly says, “ You may turn (almost) all flesh into a fatty substance, if you take flesh and cut it into pieces, and put the pieces into a glass covered with parchment, and so let the glass stand six or seven hours in boiling water. It may be an experiment of profit for making grease or fat for many uses; but then it must be of such flesh as is not edible, as horses, dogs, bears, foxes, badgers, &c.”

After having seen some of the matter found in the burial place of the Innocents at Paris, our author concluded that in some situations the same kind of substance might be easily found; accordingly he examined some of the macerating tubs belonging to anatomical schools in town, and he found that in most of them the flesh was nearly changed into this kind of fat. By the indulgence of Dr. Pegge, the anatomical professor in Oxford, he was permitted to examine the receptacle in which the bodies are deposited, after he has finished lecturing on them. This place is a hole dug in the ground to the depth of about thirteen or fourteen feet, and, to remove all offensive smell, a little stream is turned through it. Mr. Gibbes  
found,

found, on first looking into it, that the flesh was quite white, and on drawing up the first piece, he found it changed in the manner before described. From this place he has procured, he tells us, at least twelve pounds weight of a substance equal in every respect to spermaceti.

Having seen many specimens of different animals, which had been changed under somewhat different circumstances, that is, where some had been buried in dampish ground, some in wet ground, and some even in water itself, he began to suspect that the same change might be brought about in a shorter time, at least that he might determine the time necessary for it: with this view a piece of the leanest part of a rump of beef was confined in a box full of holes, which being tied to a tree near a river, was suffered to float in it. On taking this up from time to time, he perceived that it gradually got whiter and whiter, and at the end of a month it was perfectly, to appearance, changed to a mass of fatty matter. From some circumstances, he is induced to believe that it is sooner converted in running water than when it is perfectly at rest; for while this beef was exposed to the water in the river, a piece of mutton

was

was placed in a reservoir of water, and he perceived, that though the mutton was exposed for a longer time than the beef, yet that it was not so much changed.

Finding that this substance was so formed, and that he could procure large quantities of it, Mr. Gibbes made some experiments to purify it; for this purpose he took several pieces of it and melted them, and he found, though they were brought into a closer union, yet that the fœtid smell was as bad as before. After trying some unsuccessful experiments, it occurred to him, that if he could add a substance to it which would unite with the offensive parts, and not with the fat, he might then get it pure; accordingly he poured some nitrous acid upon it, which immediately had the desired effect; a waxy smell was perceived, and on separating and melting it, he got it, we are told, nearly pure. The nitrous acid, he observes, turns it yellow, but by submitting it to the action of the oxygenated muriatic acid, he has got it quite white and pure.

In the beginning of June, 1793, Mr. Gibbes buried a cow in a place where, from the rising of a river to supply a mill twice a day, it was submitted to the action of running water. On  
taking



taking this cow up in December, he found, that where the water was constantly running over it, there it was changed into a fatty substance; but where the water which had acted on the meat could not pass off, there a very disagreeable smell was sensible, and the flesh was not so much changed. A piece of this cow, that was perfectly lean, was stuck through with a stick, and fastened to the bottom of the river; this piece, he assures us, was perfectly changed into a fat matter, and had lost its offensive smell.

Mr. Gibbes adds, that he has brought about this change in a much shorter time, in the following manner: he took three lean pieces of mutton, and poured on them the three mineral acids, and he perceived that at the end of three days each was much altered; that in the nitrous acid was much softened, and on separating the acid from it, he found it to be exactly the same with that which he had before got from the water; that in the muriatic acid was not so much altered; the vitriolic acid had turned the other black.

From these experiments our author thinks it appears, that it is not at all necessary that the putrefactive fermentation should take place;  
but,

but, on the contrary, that it takes away a great deal of the flesh which might serve for the formation of a greater quantity of this waxy substance.

We come now to Mr. Gibbes's second paper on the same subject. In his first, as we have seen, he observed that the substance procured either by means of water, or of the nitrous acid, appeared to have precisely the same external characters; but, it seems, he has since observed a difference between that obtained from quadrupeds, and that which is procured from the human subject: the former, we are told, seems not disposed to crystallize, while the latter assumes a very beautiful and regular crystalline appearance.

After melting some of the matter procured from human muscle, our author plunged into it a very sensible thermometer, which soon rose to  $160^{\circ}$ ; at  $112^{\circ}$  the substance began to congeal, and at  $110^{\circ}$  became so solid, that the thermometer could not be easily taken out.

Mr. Gibbes took some of the spermaceti of the shops, and under the same circumstances plunged the same thermometer into it. It soon  
rose

rose to  $170^{\circ}$ ; a pellicle was formed at the top of it when at  $117^{\circ}$ ; and it became so solid at  $114^{\circ}$ , that the thermometer could not easily be taken out.

He dissolved a piece of the substance, which he had formed by means of water and the nitrous acid, in boiling spirit of wine; and on cooling this mixture, a great quantity of this waxy matter was separated in the form of beautiful flakes. He could not procure large crystals, but the flakes, he observes, assumed a crystalline appearance.

He put into an earthen retort some of this waxy matter, to which he added some finely powdered charcoal; and on applying a pretty strong fire, a small quantity of an oily fluid came over, which concreted on cooling; after this came over a great quantity of thick white vapours, which were very suffocating and offensive.

Into a copper retort he put a small quantity of this matter, and placed it on a common fire; there came over first a limpid fluid like water, without much smell; on the addition of more heat, there came over an oily fluid, which soon coagulated, of a firmer consistence than when put in, and coloured of a beautiful green  
by

by the copper; this last circumstance, Mr. Gibbes observes, proves that it contained no ammonia.

Having procured some very pure quicksilver, our author took a glass, which contained about ten pounds of that fluid, with which he filled it; he inverted it in a basin which contained the same fluid, and then introduced a small piece of lean meat, and also a small quantity of water. At the end of about six weeks, so great a quantity of gas, we are told, was disengaged as nearly to occupy the whole of the vessel; and the meat had assumed a white appearance.

Since the account given in his former paper of his experiments on the cow, which he had submitted to the action of running water, Mr. Gibbes has observed some facts relative to the changes which took place, and which he now describes. This cow, as we have seen, was placed in a situation where the water could come twice every day; and over it was thrown some loose earth. After it had remained some time in this place, our author used frequently to push a stick through this earth to the cow; and he observed, that every time this was done there came up a great quantity of air, after he

had suffered it to remain quiet for a short time. He has since, he tells us, had two horses and another cow placed in a similar situation; and in all of them, it seems, this disengagement of air takes place. This air, he adds, is extremely offensive.

Mr. Gibbes observes, that in the former cow the whole muscular part seemed changed; and that from the substance formed he procured a very large quantity of a waxy substance by means of the nitrous acid. Though the nitrous acid takes off the greatest part of the fœtor from the substance thus formed, yet it gives it, it seems, a yellow colour, which is with difficulty removed, and a peculiar smell, evidently similar to the smell of the acid employed, which mere washing and the addition of alkalis will not entirely remove.

Our author observes that his father, who has been indefatigable in his attempts to whiten this substance, has found a method by which it may be rendered very pure and beautiful, though not so white as the spermaceti of the shops. The process was as follows: the cow, which had lain in the water for a year and an half, was taken up, and the whole muscular part was found perfectly changed into a white matter;



matter; this was broken into small pieces, and exposed to the action of the sun and air for a considerable length of time. By these means it lost a great deal of its smell, and seemed to acquire a firmer consistence. The appearance of this substance was somewhat singular; for on breaking it, little filaments were seen running in every direction, exactly similar to the cellular substance between the muscular fibres. These pieces were then beaten to a fine powder, and on this powder was poured some diluted nitrous acid; after the acid had been on it for about an hour, a froth was formed at the top; the acid was then poured off, and the substance repeatedly washed; it was then melted in hot water, and when it concreted it was of a very beautiful straw-colour, without the least offensive smell: on the contrary, it had the agreeable smell of the best spermaceti.

May not this substance, our author asks, be applied as an article of commerce? Great quantities of it, he observes, may be obtained. It burns with a fine flame; and dead animals, which at present are of little or no use, may be changed into it. He regrets that it has not been in his power to ascertain the precise quan-

tity which may be obtained from a given quantity of flesh; but from what he has obtained, he can say that it would be very considerable. The running water carries off a great deal of it; but that, he observes, might be obviated by the addition of strainers. Moreover, he adds, that which is carried off by the water is the purest. The water over the animals, and for some distance round them, he tells us, is covered with a very beautiful pellicle, which is white in general; and sometimes refracts the sun's rays, producing the prismatic colours.

Fish, the author observes, may be also changed; and he recollects to have seen, in some old writer whose name he cannot recollect, an account of something of this kind happening in a whale. The writer states, that after this fish has been putrifying on the shore some time, the people have a secret by which they can procure and purify lumps, similar to the spermaceti which they get in the usual way.

On seeing a body opened some time ago, where there was a great collection of water in the cavity of the thorax, Mr. Gibbes observed that the surface of the lungs was covered with a whitish crust; and he remarked to a friend, that  
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he thought this crust was owing to some combinations which had taken place between the lungs, or pleura, and the serous fluid effused, similar to what he had observed between flesh and water; or that the serous fluid had acted on the coagulable matter, and had produced a similar change. He has found a curious fact mentioned by Dr. Cleghorn, which in some measure seems to agree with the observation then made. It occurs in that part of his *Observations on the Diseases of Minorca*, (p. 248) where he is speaking of abscesses in the lungs, and is as follows: “ These abscesses had some-  
 “ times emptied themselves into the cavity of  
 “ the thorax, so that the lungs floated in pu-  
 “ rulent serum; their external membrane, and  
 “ likewise the pleura, being greatly thickened,  
 “ and converted, as it were, into a white crust,  
 “ like melted tallow grown cold.” In a note Dr. Cleghorn says, “ I am now doubtful if this  
 “ crust was the pleura and external coat of the  
 “ lungs, changed from a natural state by soak-  
 “ ing in a purulent fluid; and if it was not  
 “ altogether a preternatural substance, formed  
 “ by fluids deposited on those membranes, and  
 “ compacted together by the motion of the  
 “ lungs.”

Mr. Gibbes observes, that much has been said by different writers on the subject of secretion, which at one time was supposed to depend on some peculiar property of the living principle; so that it was thought impossible to form any secretion but through the medium of secreting organs. M. Fourcroy, however, he adds, has contradicted this by the experiments where he forms bile.

Spermaceti is an animal substance, secreted in a particular species of whale, and the substance which is formed in the foregoing experiments, as far as our author can judge, agrees with it in every particular:

M. Fourcroy says, that M. Poulletier de la Salle found a crystallized inflammable substance similar to spermaceti in biliary calculi.

May not the suety matter in steatomatous tumours, our author asks, arise from something of this kind? And by attending to the various secretions of the body, by examining their composition in the healthy and morbid states of the system, may we not expect to derive great advantage, particularly when accurate experiments are applied towards the relief of disease?

Mr. Gibbes candidly observes, that some  
excuse

excuse may perhaps seem necessary for the little attention which has been paid to the accurate results in the different experiments he has described; particularly so, as the analysis of every part of the animal body, except the bones, is at present so incomplete; but he hopes that the time necessary for his medical pursuits, and the want of a complete chemical apparatus, will not render the simple facts he has related less useful.

He has not attempted, he tells us, to account for the various phenomena which appear in the experiments, because the facts seemed too few to admit of any general conclusion.



XIV. *Experiments on the Nerves, particularly on their Reproduction; and on the spinal Marrow of living Animals.* By William Cruikshank, Esq. Vide *Philosophical Transactions of the Royal Society of London, for the Year 1795.* Part I. 4to. London, 1795.

THE experiments described in this paper were made on the nerves of the eighth pair, or par vagum, and the intercostal nerves of dogs.

The par vagum arise from the basis of the brain, and passing out of the cranium, along with the internal jugular veins, are distributed to the tongue, œsophagus, larynx, heart, and lungs; and, running on each side of the œsophagus, may be said to terminate in the stomach, liver, and semilunar ganglion of the intercostals, below the diaphragm; from whence they are again distributed to the viscera of the abdomen.

The intercostals also arise from the basis of the brain, and passing out of the cranium, along with the carotid arteries, at first run by  
the

the fore part of the vertebræ of the neck, still adhering to the coats of these arteries; but having reached the chest, they leave these arteries, and run before the heads of the ribs, where, sending off branches, which pass between the ribs, they have thence been named intercostals. Several of these branches uniting, form a trunk on each side, which, running forwards towards the middle of the spine, perforates the diaphragm, and then terminates in the semilunar ganglion of the intercostals. These trunks are distinguished by the name of the anterior intercostals. The original trunks continue their course by the sides of the lumbar vertebræ; after which, they run before the os sacrum, and, approaching nearer each other as they descend, terminate before the os coccygis, in the ganglion coccygeum impar of Walther. Their branches all go to the heart, abdominal viscera, testicles in men, and ovaria and uterus in women. The trunks of these nerves are largest in the neck. In the human species the two nerves of each side are distinct; but in those quadrupeds which our author has examined, they are so intimately connected, we are told, through the whole length of the neck, as to make apparently but one nerve.

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The intercostal is the smallest nerve, and adheres so closely to the other, as to be with difficulty separated from it. They likewise have appeared to him larger in the dog, compared with his bulk, than in the human subject. The neck was the place in which he chose to divide these nerves; it was there they could be got at with least danger, a circumstance, he observes, which, by rendering an experiment more simple, makes it consequently more to be relied on; and, in order to put the animal to as little pain as possible, and make the operations short, he chose to divide both nerves at once, rather than take up time in separating them, and dividing them singly; so that, instead of four operations on each animal, he confined himself to two.

We come now to the experiments described by the author.

### EXPERIMENT I.

On the 24th of January, one nerve of the par vagum, with the intercostal nerve, were divided, in a dog, on the right side. The symptoms, consequent to the operation, the  
author

author observes, were heaviness, and slight inflammation of the right eye; breathing with a kind of struggle, as if something stuck in his throat, which he wanted to get up; fullness, and a disposition to keep quiet: the pulse did not seem much affected, nor did he lose his voice in the least. The unfavourable symptoms, it is added, did not continue above a day or two; and on the eighth day he seemed perfectly to have recovered.

## EXPERIMENT II.

On the 3d of February, a portion, about an inch long, of the two nerves of the opposite side, was cut out in the same dog: his eyes, we are told, became instantly red and heavy; his breathing was more difficult than in the former experiment; he was sick, and vomited frequently; the saliva was increased in quantity, and flowed ropy from his mouth; his pulse in the groin was about 160 in a minute; he ate and drank, however, even voraciously at times, and had stools; he never attempted to bark or howl, probably because he did not feel great pain; and yet his attention,  
it

it is remarked, was not so much disengaged from internal uneasiness, as to be excited with ordinary causes from without; in breathing, the inspirations were slow and deep; the expirations were attended with repeated jerks of the abdominal muscles, as if he wanted more effectually to expel what air was contained in the lungs. On the seventh day after this second operation, he was found dead at a considerable distance from his bed. In the dead body every thing, the author observes, seemed in a sound state, except the lungs; these contained little or no air; in consequence of which they sunk to the bottom in water, and were of a red brown colour. The inner surface of the trachea was exceedingly inflamed, and covered with a white fluid, in some places resembling pus, in others ropy, and more of the nature of mucus. The divided nerves of the right side were found united by a substance of the same colour as nerve, but not fibrous; and the extremities formed by the division were still distinguished by swellings, rounded in form of ganglions. The same appearance, we are told, had taken place with respect to the nerves of the left side; though the divided extremities seemed to have been full two inches apart; the  
uniting



uniting substance was more bloody than that of the other side. This experiment, Mr. Cruikshank observes, was made to prove that the original power of action, in the thoracic and abdominal viscera, is independent of the nerves. As he found the nerves regenerated, a circumstance never before noticed, it occurred to him that it might be objected to the reasoning, that the two first nerves were doing their office, before the two last were divided; to obviate this objection, he made the following experiment:

### EXPERIMENT III.

February 19th, he divided, at one operation, the four nerves composing the first class, in a dog. His eyes, it is observed, became instantly dull and heavy; he tottered as he walked; foamed at the mouth; vomited two or three times; breathed with excessive difficulty; his inspirations were long and deep, his expirations short and sudden, but not attended with the repeated jerks of the abdominal muscles as in the last animal; he barked loud every time he threw out the inspired air from the lungs; the pulse was quicker than before the operation.

operation. Next morning about half after eight, Mr. Cruikshank found him apparently dead; but on examining more attentively, perceived that he breathed still, though exceedingly slowly; his pulse was gone; he felt cold; and his limbs were stretched out. On being placed near the fire, he began in a few minutes to breathe distinctly, and the heart now and then gave a pulsation; in about four hours he seemed to have got to the same state the operation first left him in, and barked at every expiration, his pulse beating then fifty in a minute. About four in the afternoon he died, having survived the operation twenty-eight hours. The lungs in the dead body were found loaded with blood, but not so much, Mr. Cruikshank observes, as to carry them to the bottom in water. The trachea was not inflamed. The nerves of the right side, from which a portion had been cut out, seemed to have undergone little alteration; they were only a little more vascular than usual, and had the rounded swell where they had been divided. The nerves of the left side, which had retracted but little, and had been only divided, had their extremities covered with a plug of coagulable lymph.

Mr. Cruikshank suspected that the reason of the first dog's dying so soon was, that none of the nerves had yet acquired the power of performing their former offices; and that, were the operations performed at a greater distance of time, the animal would recover. With this idea he was led to repeat his experiments, allowing a greater interval to take place between the first and second.

#### EXPERIMENT IV.

March 6th, he repeated Experiment I. on a large dog. His right eye, we are told, seemed instantly affected, looked dull and inflamed; he coughed and breathed with some difficulty; the secretions from the salivary glands were much increased; he had tremors; these, however, Mr. Cruikshank attributes partly to fear, as on caressing him they disappeared. He ate and drank very well, and had stools. Most of these symptoms, it seems, continued but a few days, the eye becoming more clear, and the difficulty of breathing hardly perceptible; he vomited, but only after eating, a circumstance, the author observes, which often takes place in  
dogs

dogs in perfect health, from devouring their food too greedily. Thus he continued for three weeks; the external wound had healed, almost by the first intention; he ate greedily, and had perfectly recovered. Mr. Cruikshank supposed the regenerated nerves might now be performing their offices.

#### EXPERIMENT V.

March 27th, our author repeated Experiment II. on the same dog, but did not remove quite so much of the nerves. The animal, we are told, was stupid for a minute or two, and gaped for breath; but in a few minutes more these symptoms went off; in a quarter of an hour after he ate some boiled meat, with his usual avidity; all the symptoms of the preceding operation again took place, and in the same order. The vomiting and difficulty of breathing were rather more considerable; he ate and drank, notwithstanding, and had stools. The convulsive jerks of the abdominal muscles, which hardly took place in the last experiment, were observed in this, during expiration, but were not constant, as in the first dog. On the

15th of April he was nearly as well, it is observed, as before the operations, only he was leaner, and perhaps weaker, from the confinement, as well as from the operations. Mr. Cruikshank wished to see the state of the nerves; an artery was opened in the groin, and the animal expired in a few seconds.

On examining the dead body, the viscera were all, to appearance, found. The nerves of the right side, which had been divided, were found firmly united; having their extremities covered with a kind of callous substance; and this leads the author to think the regenerating nerve, like bone in the same situation, converts the whole of the surrounding extravasated blood into its own substance. The nerves of the left side were also perfectly united; but the regenerated nerves were smaller than the original; a circumstance which the author ascribes to the quantity of extravasated blood having been less. He observed too, that they did not seem fibrous, like original nerves; but the recollection that the callus of bone is dissimilar to the original bone, quieted, he tells us, whatever doubts could arise from this circumstance.



## EXPERIMENT VI.

April 19th, our author divided the spinal marrow of a dog, between the last vertebra of the neck and the first of the back. The effects of this operation were as follows: the muscles of the trunk of the body, but particularly those of the hind legs, appeared instantly relaxed; the legs continued supple, like those of an animal killed by electricity; the heart, on the operation being performed, ceased for a stroke or two, then went on slowly and fully, and, in about a quarter of an hour after, the pulse was 160 in a minute: respiration was performed by means of the diaphragm only, which acted very strongly for some hours. The operation was performed about a quarter before twelve at noon; about four in the afternoon the pulse was ninety only in a minute, and the heat of the body exceedingly abated; the diaphragm acting strongly, but irregularly. About seven in the evening, the pulse was not above twenty in a minute, the diaphragm acting strongly, but in repeated jerks. Between  
twelve

twelve at night and one in the morning, the dog was still alive; respiration was very slow, but the diaphragm still acted with considerable force. Early in the morning he was found dead.

This operation, Mr. Cruikshank observes, he performed from the suggestion of Mr. Hunter, who had observed, in the human subject, that when the neck was broke at the lower part, (in which cases the spinal marrow is torn through) the patient lived for some days, breathing by the diaphragm. This experiment, the author adds, shows, that dividing the spinal marrow at this place on the neck, if below the origin of the phrenic nerves, will not, for many hours after, destroy the animal; and was preparatory to the following experiment:

## EXPERIMENT VII.

April 26th, the author divided all the nerves of the first class, in a dog. The principal symptoms, he observes, of Experiment III. took place. Soon after, he performed, on the same animal, the operation of Experiment VI.

when the symptoms peculiar to that operation also took place, whilst those peculiar to Experiment III. disappeared. His respirations, we are told, were five in a minute, and more regular than in Experiment III.; the pulse beat 80 in a minute. Five minutes after, Mr. Cruikshank found the pulse 120 in a minute; the respiration not altered. The operation was performed about two in the afternoon, and at three quarters after five, the respirations were increased to fifteen in a minute; the pulse beating 80 in the same time, and very regularly: the breathing, it is remarked, seemed so free, that he had the appearance of a dog asleep. At a quarter before eight, the pulse beat 80, and the respirations were ten in a minute. At three quarters after ten, the respirations were eight in a minute, and the pulse at 60. The animal heat was now exceedingly abated; heat was applied to the chest, after which he breathed stronger, and raised his head a little, as if awaking from sleep. At half after twelve the breathing was strong, and twelve in a minute, the heart beating forty-eight in the same time, slowly, but not feebly. He shut his eyelids when they were touched; shut his mouth on its being opened; and raised his head a little,

but

but as he had not the use of the muscles which fix the chest, he did it, we are told, with a jerk. Between four and five o'clock in the morning, the respirations were five in a minute, and the pulsations of the heart exceedingly slow and weak. He died about six in the morning, having survived the operation sixteen hours.

This experiment, Mr. Cruikshank tells us, he made from the suggestion of Mr. Hunter, with a view to obviate an objection raised against the reasoning drawn from the three first experiments. It was urged, it seems, that though by these experiments he had deprived the thoracic and abdominal viscera of their ordinary connexion with the brain, yet, as the intercostals communicated with all the spinal nerves, some influence might be derived from the brain in this way. This experiment removed also the spinal nerves, and consequently this objection, the author thinks, was obviated.

As he had found, by the two last experiments, that dividing the spinal marrow in the lower part of the neck did not immediately kill, although instant death was universally known to be the consequence of dividing it in the upper part of the neck, he expressed, he

tells us, his surprise to Mr. Hunter, that the spinal marrow should, according to modern theory, be so irritable in the one place, and so much less so in the other.

Mr. Hunter told him, that from the time he first observed that men, who had the spinal marrow destroyed in the lower part of the neck, lived some days after it, he had established an opinion, that animals, who had the spinal marrow wounded in the upper part of the neck, did not die from the mere wound; but that in dividing it so high, we destroyed all the nerves of the muscles of respiration, and reduced the animal to the state of one hanged; whereas in dividing it lower, we still left the phrenic nerves, and allowed the animal to breathe by his diaphragm. Our author conceived, that if this opinion were well founded, though dividing the spinal marrow in the lower part of the neck does not kill instantly, whilst the phrenic nerves are untouched; yet if the phrenic nerves were first divided, and then the spinal marrow in the lower part of the neck, the consequence would be the same as if it had been divided in the upper part. With a view to ascertain this point, he made the following experiment:



## EXPERIMENT VIII.

By detaching the scapulæ of a dog from the spine, and partly from the ribs, he got at the axillary plexus of nerves, on both sides, from behind. He separated the arteries and veins from the nerves, and passed a ligature under the nerves, close to the spine. He thought he could discern the phrenic nerves, and instantly divided two considerable nerves going off from each plexus. The action of the diaphragm, he observes, seemed to cease, and the abdominal muscles became fixed, as if they had been arrested in expiration, the belly appearing contracted. The respirations of the animal, we are told, were now about twenty-five in a minute, the pulse beating a hundred and twenty. As our author was not willing to trust the experiment to the possibility of having divided only one of the phrenics, (which he afterwards found was really the case) and some different nerve instead of the other, after carefully attending to the present symptoms, he divided all the nerves of the axillary plexus, of each

side. The ribs, he observes, were now more elevated in respiration than before; the respirations were increased to forty in a minute, the pulse still beating a hundred and twenty in the same time. Finding that respiration went on very easily without the diaphragm, in about a quarter of an hour after dividing the axillary plexus of each side, he divided the spinal marrow, as in Experiment VI. All the flexor muscles of the body now seemed to contract, and instantly to relax again; and this animal died as suddenly as if the spinal marrow had been divided in the upper part of the neck.

Mr. Cruikshank opened the chest, and found the heart had ceased its motion; he immediately introduced a blowpipe into the trachea, below the cricoid cartilage, and inflating the lungs, imitated respiration. The heart now began to move again, and in about three minutes was beating seventy in a minute. He recollected that there was still a communication between the brain and the thoracic and abdominal viscera, that the par vagum and intercostals were entire, and turning to the carotids, divided the nerves. He then went on inflating the lungs as before, and the heart, which had stopped, began to move again,  
beat

beat seventy in a minute, and continued to do so for near half an hour after the animal had seemingly expired. These appearances, the author observes, were not confined to the neighbourhood of the heart; one of the persons who assisted him in the experiment, cried out once, that he felt the pulse in the groin. Mr. Cruikshank now ceased to inflate the lungs, and presuming that he could easily reproduce the heart's action, allowed three minutes to elapse. On returning to inflate the lungs, he found the heart had now lost all power of moving; and that irritating the external surface with the point of a knife, did not produce the smallest vibration. He then irritated the phrenic nerves with the point of a knife, and found that the diaphragm contracted strongly as often as the nerves were irritated. He next irritated the stomach and intestines, which also renewed their peristaltic motions. He likewise irritated the par vagum and intercostals, about an inch above the lower cervical ganglion of the intercostal; and the œsophagus contracted strongly through its whole length: but the heart continued perfectly motionless. On dissection, he found a small branch of a nerve, running down from the second cervical to join the

the

the phrenic of the right side, but he considered it as too insignificant to have any effect on the experiment.

This experiment, Mr. Cruikshank remarks, confirms those made by Mr. Hunter, in which he recovered animals by inflating their lungs, and on which his method of recovering apparently-drowned people principally rests. It shows also, in his opinion, that respiration is the prime mover of the machine; and it takes off, he thinks, whatever objections might have been raised, from the animals, upon which he made his experiments, having the connexion with the brain entire (as the par vagum and intercostals were not divided) since here the same thing took place in these experiments where nerves could have no effect.

XV. *An experimental Inquiry concerning the Reproduction of Nerves.* By John Haighton, M.D. Vide *Philosophical Transactions of the Royal Society of London, for the Year 1795.* Part I. 4to. London, 1793.

IN the preceding paper the reader has seen an account of the experiments made by Mr. Cruikshank, in the year 1776, and which induced him to believe that a nerve, when divided, possesses, like the bones and some other parts of an animal body, the power of repairing its loss with a substance resembling itself. Since that period different opinions\* have prevailed among anatomists and physiologists concerning the nature of the new-formed substance; some maintaining that it is similar to the original nerve, while others suppose it to be totally different.

\* Vide Fontana *Traité sur le Ventre de la Vipere . . . sur la Reproduction des Nerfs, &c.* Tom. II. 4to. Florence, 1781; and Arnemann *Versuche ueber das Gehirn und Rückenmark.* 8vo. Goettingen, 1787.

When



When opinions so opposite to each other prevail on a point, which experiment seems so fully adequate to decide, we are naturally led, observes the ingenious author of the paper now before us, to take a view of the manner in which the experiments were conducted, and consider the criterion to which each party has appealed.

There are only two tests, Dr. Haighton remarks, which seem to offer themselves, and from which any degree of judgment can be formed. These are, either a minute and careful examination of the new-formed substance in an anatomical way, and an accurate comparison of it with the original nerve; or, a cautious attention to the function of that nerve, by which we see the loss of it from the division, and the return of it from the reunion of the divided parts.

Those who have subjected this matter to the test of experiment, have made their appeal to the first criterion; and have either affirmed or denied the reproduction, according as they thought the new-formed part either agreed with or differed from the original nerve.

This criterion, continues our author, certainly supposes, that anatomy is fully competent

to determine what is the precise structure of nerves, what are the nature and characters of ultimate nervous fibres, and by what mechanism or power they execute their allotted function. It supposes likewise, that anatomists are perfectly agreed upon this matter; and that those who make their appeal to anatomy, have admitted a common standard of comparison, by which they allow their experiments to be judged; but no position, Dr. Haighton contends, can be more remote from fact than this is; for while some think ultimate nervous fibres are constructed to act by tremors, others believe them to be hollow tubes. Nor is the difference of opinion, he observes, less, respecting the appearances which they exhibit on being viewed by a microscope: one eminent physiologist (Dr. Monro) supposing the ultimate nervous fibres to be “serpentine and convoluted;” while another ingenious inquirer, (Abbé Fontana) thought at one time that the fibres were composed of cylinders, with bands twined around them, in a spiral direction, till subsequent examination convinced him, that this appearance had its origin in an optical deception, and that their true direction was that of “parallel winding fibres.”

As

As it appears then, that microscopical observers neither agree with each other on this subject, nor with themselves, our author thinks it fair to conclude, that ocular inspection cannot be admitted as a fair appeal, from which we can determine whether the substance which unites the extremities of divided nerves is of the same nature as the original nerve. He therefore resolved, he tells us, to submit his inquiries to a test less doubtful and fallacious; and as such a test was not to be found within the pale of anatomy, he was induced to try whether the resources of physiology could not furnish him with what he wished.

From physiology, he observes, we learn, *that if the action of a nerve be suspended by a division of it, and if that action be recovered in consequence of an union of its divided extremities, such medium of union must possess the characters and properties of nerve.* He had therefore only to determine, what nerves appeared the most favourable for the experiment, and pursue the position just stated to its ultimate consequence. “ I know not,” says he, “ whether my choice “ was judicious, but I determined on the eighth “ pair.”

The first step he took in this inquiry, was  
to

to ascertain *what effects will arise from the division of both of these nerves, together with that branch of the great sympathetic nerve accompanying and strongly adhering to them.*

### EXPERIMENT I.

A dog being properly secured, and a convenient incision made on the fore part of the neck, our author divided both the nerves of the eighth pair; the animal became immediately restless and uneasy, betraying symptoms of great distress about the stomach, which continued eight hours, when he died.

Though the result of this experiment is perfectly agreeable to what other experimental physiologists have stated, Dr. Haighton thought it of importance, he tells us, to the present inquiry, to give it confirmation by farther experiment. He therefore repeated it on two other dogs, one of which survived it three days, the other only two.

From these experiments, he observes, we learn, that the action of these nerves was suspended, and that those vital organs which received

ceived their nervous energy from this source, had their functions arrested, so that death followed as a necessary consequence.

He is aware, it may be said here, by way of objection, that a violent shock had been suddenly given to the machine; and that the animal perished rather from the sudden deprivation of the nervous influence, than from its absolute loss; and that if the same quantity had been abstracted in a more gradual way, the animal might have survived it. How little validity there would be in such an objection the following experiment, he thinks, will evince.

## EXPERIMENT II.

Another dog being procured, Dr. Haighton divided only one of the nerves of the eighth pair. He was surprised to see how slightly he was affected from it; for, excepting a little moroseness, there was scarcely any alteration, we are told, perceptible, so that in a few hours after the operation he took food as usual. On  
the



the third day, our author divided the other nerve; but the same symptoms immediately supervened here as followed the division of both nerves in the former experiments: the animal continued in a state of restlessness and anxiety, with palpitation and tremors, until the fourth day, when he died.

The event of this experiment, the author observes, differs in nothing more from the former, than that the fate of the animal was suspended a little longer, but the ultimate effect was exactly the same: hence, he contends, that, in the first experiments, *the death of the animal is not to be imputed to the mere sudden deprivation of nervous energy, but to its absolute loss.*

Wishing next to determine whether, by lengthening the interval between the division of the two nerves, a few days more, the life of the animal could not be protracted to a greater length, or even saved, Dr. Haighton made the following experiment:

## EXPERIMENT III.

Having divided one of the nerves of the eighth pair, and waited the lapse of nine days, he divided the other. The same symptoms, he observes, came on now as in the last experiment, but scarcely so violent. The only kind of food the animal would take was milk, and that in small quantities; and this, it seems, always produced great uneasiness at the stomach, with symptoms of indigestion. In this state he continued thirteen days, and then died, much emaciated.

From this dog having lingered so long, Dr. Haighton tells us he was beginning to entertain hopes of his recovery, and had that eventually happened, he doubted much whether, even under the present uncertainty of things, he could have resisted the temptation of ascribing such recovery to the reproduction of the nerves; but the event, he adds, put a stop to his speculation.

Having thus, as he thinks, proved his first position, that whether the eighth pair of nerves  
be

be divided in immediate succession, so as to deprive an animal of their influence suddenly, or whether this deprivation be effected in a more gradual way, the consequences are in the end equally fatal; Dr. Haighton next endeavours to avail himself of this fact in the solution of the problem now before us. If, he observes, the substance of nerve be reproduced, certainly a period longer than the above must be necessary for the process; but to mark the precise point of time when the line is to be drawn, would, he adds, require the sacrifice of more animals than a question of mere curiosity could justify. He therefore contents himself with giving a general answer to the question, and inquiring whether, by suspending the division of the second nerve for a much greater length of time than was done in the two last experiments, the existence of the animal could be preserved.

#### EXPERIMENT IV.

Having procured another dog, and divided one of the nerves of the eighth pair, Dr.

Haighton allowed six weeks to elapse before the other was cut through. This division of the corresponding nerve, it is observed, evidently deranged the animal; but in a much less degree than in the former experiments. For some days, we are told, he refused solid food, but took milk; afterwards he ate solid food in small quantities, and near a month had passed away before he fed as usual. The actions of the stomach, it is added, were for a long time evidently deranged, so that he was continually harassed with symptoms of indigestion; and six months had nearly elapsed before he recovered his health, though during five months of the time he took his usual quantity of food.

Now, to what cause, our author asks, are we to impute the recovery of the animal in this experiment? The most probable one he thinks is, that in the interval of six weeks the first nerve had been reproduced; so that the actions of the organs depending upon this nerve, though somewhat disturbed, were not suspended: and afterwards, as the union of the second nerve advanced, and the reproduction of the first became more perfect, the vital organs gradually recovered their healthy state.

Dr. Haighton kept this animal nineteen months,

months, during the greatest part of which time he performed the office of a yard dog. And here he has thought it proper to observe, that in all the experiments, the voice was totally lost on the division of the second nerve. This effect, anatomists, he adds, will easily understand, from recollecting that the recurrent branches of the eighth pair, which are the true vocal nerves, originate below the part where the trunks of the eighth pair were cut through; consequently those nerves were themselves in effect divided. Now it deserves to be remarked, continues our author, that the dog's voice returned in proportion as his general health improved; and that in about six months he could bark as strongly as before, but that the pitch of his voice was evidently raised.

From this experiment, Dr. Haighton is strongly inclined to believe that there must have been a true reproduction of the nerve; yet he does not contend, that if the part of union were examined by an anatomical eye, such reproduction would be very evident. On the contrary, he is persuaded that anatomy can determine only the presence and existence of an uniting medium; but it is the province of physiology, he observes, to decide whether the me-



dium of union possess the characters, and perform the function, of the original nerve.

He is aware that the evidence of reproduction, as resting on this experiment, may not be sufficient to obviate every doubt which reflections upon this subject may probably suggest; and that some may be disposed to object to the possibility of the stomach and larynx having received an additional supply of nervous energy from other sources; especially as the former of these is known to receive nerves from the great sympathetic as well as from the eighth pair; and the larynx receives a branch from this (the eighth) pair, which arises from the trunk above the part where the division was made. Our author candidly acknowledges that the familiar analogy of the vascular system, where collateral branches are enlarged from the obliteration of a principal trunk, tends farther to give weight to these doubts.

To remove these seeming difficulties by anatomical investigation, or by directing his views to any changes that might be induced on the anastomosing nervous filaments, would, he observes, be an undertaking not less tedious in its execution than unsatisfactory in its result; for there would still remain room for opposite opinions :

nions : and while some would argue that these anastomosing filaments were become evidently enlarged, others would contend that they had not suffered the slightest change.

Now, our author, as we have seen, has already expressed his distrust of those decisions which are founded on an appeal to the eye, seeing that anatomy has yet to explain by what mechanism or structure these organs perform their office. He therefore has preferred an appeal to the functions of these parts, inquiring whether, in the experiment in which the dog survived the division of the second nerve of the eighth pair after an interval of six weeks, it was effected by the reproduction of the first divided nerve, or in another way ?

There are only two possible answers, he observes to such a question ; these are, that either the functions of the stomach, larynx, &c. were carried on by anastomosing nerves ; or that the united nerves had recovered their original importance.

If the first be contended for, this consequence, he thinks, ought to ensue, viz. that the eighth pair should now be entirely useless, and both of them may be divided a second

time, without injuring any of the functions of the animal.

If the last be granted, it must, he contends, of necessity follow, that the medium of union possessed the same properties as the original nerve.

He has thus, he thinks, circumscribed the field of inquiry, and drawn the question into so narrow a compass, that it is in the power of a single experiment to prove either the affirmative or negative. If now the eighth pair be divided a second time in immediate succession, and the animal sustain it with impunity, he conceives it right to conclude, that the actions of those organs, which originally were carried on through the means of the eighth pair, are now performed by other channels, and that the true substance of the nerve is not reproduced. But, on the contrary, if the animal die in consequence of it, then he thinks it equally just to infer, that the new-formed substance is really and truly *nerve*, because we know of no other substance which can perform the office of nerve.

He relies, therefore, he tells us, upon the following, and considers it as his *experimentum crucis*.

## EXPERIMENT V.

Having the dog in his possession upon which he divided the eighth pair of nerves nineteen months before, he cut through both of them now, in immediate succession. The usual symptoms, we are told, were immediately induced, and continued until the second day, when he died.

After death Dr. Haighton carefully dissected out these nerves, and has preserved them as evidence of his success. He thinks he has now answered the question he has proposed to himself, and proved that nerves are not only capable of being united when divided, but that the new-formed substance is really and truly nerve.

XVI. *Description of a Human Male Monster, with Remarks.* By Alexander Monro, M.D. F.R.S. Edin. Fellow of the Royal College of Physicians, Professor of Medicine, Anatomy, and Surgery in the University of Edinburgh, Fellow of the Royal Academy of Surgery in Paris, &c. Vide *Transactions of the Royal Society of Edinburgh*. Vol. III. 4to. Edinburgh, 1794.

THE monster here described, of which the mother was delivered after the birth of a complete child at the full time, had its proper membranes and a placenta, with a short umbilical cord.

The following parts were wanting in it, viz. the bones of the head; the brain, with the organs of sight, hearing, smell, and taste; the neck; about one half of the ribs; the larynx, trachea, and lungs; the heart; the pharynx, œsophagus, and stomach, with all the small intestines, except the end of the ilium; the anus; the liver, spleen, pancreas, and omenta; the renal glands; termination of the ureters; both



both arms; both patellæ; and several of the bones of the feet and toes.

A round opening, which led to a thimble-like cavity, shut at its bottom, had some distant resemblance to the mouth.

The soft parts of the trunk were supported by sixteen vertebræ, six ribs, an os sacrum, and two ossa innominata. The legs had each an os femoris, tibia, and fibula, with an imperfect number of the bones of the feet.

The umbilical cord was connected at nearly the usual height above the ossa pubis.

The penis, covered with a large preputium, had the usual situation and structure.

The lower part of the trunk contained an intestinal tube, shut at its beginning, and composed of an upper part, four inches long, resembling the end of the ilium; for it terminated in the side of an intestine, resembling the caput coli, with its appendix vermiformis. From this place, to its lower end, the great intestine measured thirteen inches; and the end of the rectum terminated in the back part of the urinary bladder. The rectum contained a viscid semipellucid mucus; but nothing like the meconium.

In the mesentery and mesocolon, there were  
about

about a dozen conglobated lymphatic glands, of the usual shape, colour, and consistence; from which it appeared that the intestines were provided with lacteal vessels, and it is therefore, observes our author, not to be doubted that the other parts of the body were furnished with lymphatics, or that there was an absorbent as well as circulating system in this monster.

At the upper part of the trunk, covered by the ribs, there were two kidneys of a large size, with a pelvis and ureter to each. Both were shut at their lower ends, and had no communication with a small sac, which, in situation and structure, resembled the urinary bladder, and had an urachus coming from it.

There was only one testis, situated, in the usual manner, on the left side.

The urethra was wanting from within an inch of the urinary bladder to within an inch of the extremity of the penis.

The spinal marrow was of a conical shape, with the top or small part of the cone at its upper end, and at its lower end it formed a cauda equina. From its two ends and sides it sent off eighteen pair of nerves, which, at their origin and in their progress, we are told, were nearly as large as in a perfect foetus.

The

The umbilical chord, Dr. Monro observes, was nearly proportioned to the bulk of the monster; and, at the umbilicus, consisted of one vein and two arteries, within which he found red blood. The vein, he adds, was more capacious than both arteries together; and as soon as it entered the abdomen, was divided into various branches, which were dispersed upon all parts of the body.

Vessels, our author remarks, every where accompanied the branches of the umbilical vein, corresponding with them in size as well as situation; and joining together, formed trunks, from which, at the sides of the pelvis, two vessels were continued, one of them on each side of the vesica urinaria and urachus, to the umbilicus, which they perforated, and then went, along the umbilical cord, towards the placenta, resembling the umbilical arteries.

Dr. Monro regrets, that before he received this monster, the injection of its placenta had been intrusted to some person who had managed it so negligently, that nothing could be determined as to the distribution or communication of the vessels of the placenta with each other, or with those of the placenta of the complete child, or with those of the mother.

We

We must refer our readers to the work itself for four tables illustrative of this description, all the figures of which are of the natural size. One of these figures\*, which represents the fore view of it entire, we have taken the liberty to copy on a reduced scale of one third of the size of the original engraving, for the sake of conveying to our readers a more accurate idea of this singular production than can possibly be derived from a verbal description alone.

We come now to the very judicious and interesting remarks of the learned author on productions of this kind. These remarks are arranged under distinct heads or sections. The first consists of preliminary observations. In the second and third sections he treats of the direction and the causes of the motion of the blood in this monster; in the fourth he considers its nervous system; in the fifth, the duration of its life; and in the seventh and last section he considers the time at which this monster

\* See Plate II. fig. 3, in which *a, b, c,* refer to a circular mass, more than two inches thick, which supplied the place of head, trunk, and arms; *d,* to a thimble-like cavity, somewhat resembling the mouth; *e,* to the umbilical chord; *f, g,* to the penis and preputium; and *h, i, k, l, m, n,* to the thighs, legs, and feet.

must have acquired the structure which has been described.

Monsters wanting the head, heart, and lungs, and in almost every other respect agreeing with that above described, have, he observes, been mentioned by different writers, particularly by Mery and Winslow\*; and Roederer†, he adds, has given a full description of a monster, in which one small muscular sac only was found, instead of a complete heart, communicating with the continuation of one of two veins which were found in the umbilical cord; but our author is of opinion that the real course of the blood, or the causes of its motion, have been misapprehended by all those writers.

Mery, he observes, thinks the blood of the foetus must have been moved by the motion of the heart of the mother, and considers the want of the heart in such monsters, as a strong confirmation of the opinion he entertained, that there is a circulation of the blood carried on between the mother and the foetus ‡.

Winslow,

\* Mem. de l'Acad. des Sciences, 1720 and 1740.

† Comment. Societ. R. Scient. Gotting. Tom. IV. 1754.

‡ Mery, Mem. de l'Acad. des Scien. 1720. 1re Reflexion. “ Sa vie n'a pu avoir pour principes que la respiration ”



Winflow, remarks our author, from not having found any red blood in the vessels of the fœtus, nor traced within it the branches of the umbilical vein, but those only, as he imagined, of the vessel he called aorta, and which he thought performed the office of an artery, was led to the supposition, that, instead of a circulation, there was only a sort of progression of the colourless blood, or lymphatic humour, to the capillary extremities of the arterial ramifications, and that it transfused, by little and little, and very slowly, into the cellular texture of all the parts, and perhaps, at last, passed through the pores of the skin, in form of moisture\*.

Dr.

“ ration et le mouvement circulaire du sang de sa mere.”  
 And in the Historical part of the same volume, “ Le défaut  
 “ du cœur prouve que le sang qui a circulé dans ce fœtus  
 “ ne recevoit son impulsion que du cœur de la mere....  
 “ M. Mery a toujours soutenu la circulation reciproque  
 “ entre la mere et le fœtus, et telle que le fœtus est toujours  
 “ comme un membre de la mere.”

\* Winflow, Mem. de l'Acad. des Scien. 1740.

P. 588. “ La veine ombilicale, s'étant écartée du cor-  
 “ don dès son entrée dans le ventre, y formoit un tronc fort  
 “ court, qui montoit tout droit, et s'implantoit à la base du  
 “ bouton cutané, s'adossant là avec le tronc d'un autre vais-  
 “seau

Dr. Roederer\* not only applies the term of vena cava to the large vein with which the umbilical vein is joined to the heart, but describes the cava as ascending from the abdomen to the

“seau de pareille grosseur, qui sortoit de la même base, et  
 “qui étant d’abord courbé vers en bas, descendoit derriere  
 “le paquet des intestins, à peu près comme le tronc de la  
 “portion inferieure de l’aorte, et se distribuoit ensuite en  
 “plusieurs branches, de la maniere que je dirai ci après.”

P. 590. “On ne voyoit pas une goutte ni aucune apparence de sang rouge dans toute l’étendue du corps de cet enfant, ni aucun vestige de vaisseaux veineux.”

P. 600. “Hors la petite portion de la veine ombilicale après son entrée par le nombril, je n’ai trouvé dans tout le corps de cet enfant aucun vaisseau veineux, ni le moindre vestige, soit de tronc, soit de ramifications de veines.”

P. 604. “Mais à l’égard de la circulation intrinseque dans les parties mêmes de ce demi-corps, l’absence, ou la privation totale, de vaisseaux veineux m’a fait conjecturer, qu’au lieu de circulation proprement dite, il n’y a eu qu’une espece de progression ou trusion jusqu’aux extrémités capillaires de toutes les ramifications arterielles, et que là ce sang lymphatique transudoit peu à peu, et très lentement dans le tissu cellulaire de toutes les parties.... Et, peut-être, passoit par les pores externes de la peau en maniere de moiteur. Je n’avance tout ceci que comme des pures conjectures.”

\* Com. Soc. R. Sc. Gotting. tom. iv. com. 4.

thorax\*. In like manner, he not only applies the name aorta to the vessel which accompanies the continuation of the umbilical veins; but speaks of the aorta as ascending from the thorax to the head†, and sending off the subclavian and carotid arteries; and remarks, that canals proper to the latter were wanting‡. And he observes, that the aorta, after descending, as usual, between the crura of the diaphragm,

\* P. 142. “Duplicem autem umbilicalis funis venam largitur. Altera minor.... cum vena cava, ex abdomine ascendente confluit.”

† P. 144. “Arteria magna, quam aortam vocant.... ex abdomine in thoracem ascendit. In thorace eandem pene directionem.... fervans, nulloque cum corde canali confluens, sola et a corde distincta, iter suum absolvit. Nullus proinde ex aorta arcus formari potest, sed laterales rami ex recto aortæ trunco emittuntur. Sunt isti rami qui descripti sequuntur.

“In regione costæ primæ levissime descendentes arteriæ subclaviæ nascuntur, ex quibus vicissim triplex alia ramorum species oritur; quarum prima ad cervicis musculos ascendit. Porro truncus aortæ per semi pollicem postquam progressus est in duos ramos dividitur, duas nempe arterias carotides, quæ ad altitudinem laryngis sine infiori ramo ascendunt.... Ascendit autem carotis dextra, &c.—Ad latus tandem laryngis carotis communis in sex omnino ramos dividitur.”

‡ P. 166. “Canalis pro arteria carotide deest. Carotis per amplum foramen lacerum ad cerebrum tendit.”

gave off the mesenteric, renal, lumbar, and iliac arteries; and that the left iliac artery sent off an umbilical artery: and concludes his description in the following words: “ Ita quidem, si arteriæ umbilicalis dextræ, arteriæ-que celiacæ defectus.... excipiat, vix ab usitata fabrica aberrans arteria aorta in abdomine distribuitur\*.”

After an elaborate description of the several parts of the monster, Dr. Roederer proposes the cause of the motion of its humours, in the following words:

“ Motus qui.... humores agitat, causa indagatur.... Ast aliquis, lentus licet, parafitici fetus humores motus agitavit.... A corde, fueto motore, repeti iste motus nequit, neque multum auxilii propulsus in uterum maternum sanguis ferre potest. Præter vero istum levem debilemque.... Ipsa vasorum actio, five contrahendo agat, five attrahendo, vi illa capillaribus tubis familiaris, præcipuum humoribus motum imperiri debet.... Accedunt forsitan et aliæ in fetu nostro causæ incognitæ, ipsa fortasse a calore excitata fluidorum agitatio aliaque †.”

\* Comin. Soc. R. Scient. Gotting. Tom. IV. p. 155.

† Ibid. p. 212.

But as to the direction in which he supposed the humour to be moved, Dr. Roederer, observes our author, says nothing, and therefore leaves the reader to judge of his opinion, from the foregoing description of the blood vessels.

Dr. Monro thinks, that to the opinions of all these authors, when fully considered, we shall find insuperable objections. Thus, without saying in objection to that of Mery, that it is so far from being certain, that there is a circulation of red blood between the mother and foetus, that the contrary opinion is the most probable, we cannot, he observes, conceive, although the anastomoses of the uterine with the placental vessels were proved, that the mere impulse of the blood in the minute arteries should have carried the blood, not only into the trunks, but through all the capillary branches of the vessels of the foetus, and again back from these to the placenta, and from its umbilical arteries into the umbilical veins and veins of the uterus.

Dr. Monro contends, that the opinion of Winslow is far more unsatisfactory than that of Mery. He observes, that in the first place, it cannot be applied to the monster described by Mery, or to that which is the subject of the present paper, where there were two sets of vessels;



vessels; and, in the next place, that Winslow was so far from tracing distinctly the joining of the umbilical vein with the vessel he calls aorta, that he describes it as merely *s'adossant* with the trunk of the aorta\*; and farther, that although he repeatedly affirms there were no venous vessels in any part of the body of the monster, yet his description of the vessels of the kidney will not be found to correspond with his general assertion; for he describes a vessel which, indeed, he calls arterious, but which began on the fore part of the belly above the navel, at the place where the small portion of the umbilical vein terminated in the cavity of the cutaneous button, from which various branches were sent into the kidney at its convex part, and from its concave part different arteries, he says, came out in an extraordinary manner †.

\* See Mem. de l'Acad. des Sciences, 1740, p. 588; or the note in page 176 of the présent volume.

\* P. 602. “ Ce tronc artériel qui étoit comme la portion inferieure de l'aorte descendante, au lieu de tenir la route naturelle en arriere le long des vertebres, en étoit ici très éloigné. Il commençoit sur le devant du ventre au dessus du nombril, à l'endroit où se terminoit la petite portion de la veine ombilicale.... Il jettoit des branches dans la masse du rein par ..... sa convexité. Il sortoit de la concavité plusieurs artères.”

Upon the whole, as the umbilical cord is not said to have been uncommon in size or structure; as there were two sorts of vessels connected with the kidney; as it is so improbable, as to be incredible, that the fœtus received arteries without corresponding veins, or that there was merely a protrusion of the humours, and exudation of them, without circulation, Dr. Monro has no doubt that Winslow, especially as he did not inject the vessels of the umbilical cord, has mistaken the continuation of the umbilical veins, and the branches of the vessels he calls aorta, for branches of the same vessel; and as the monster he examined agreed very nearly, in all other respects, with that which is the subject of the paper before us, our author apprehends it must have agreed likewise in having two kinds of blood-vessels, arteries, and veins.

Dr. Roederer rejects the opinion of Mery, that the blood of the fœtus is circulated by the heart of the mother, and supposes, that capillary attraction, heat, and some activity of the vessels, may contribute to its motion. But as he applies the term aorta, not to the continuation of the umbilical vein, but to the other principal vessel of the monster, and describes it

it as sending branches downwards from the abdomen to the inferior extremities, and upwards from the thorax to the head, and applies the name of carotid arteries to two of these branches, with the additional remark, that the canales carotici were wanting, it will, our author thinks, appear evident from these circumstances, and from what he observes in his second section, to which we now come, and in which he considers the direction of the blood in this monster, that Dr. Roederer has mistaken the direction in which the blood was moved and circulated.

As there were two kinds of vessels, arterious and venous, in the umbilical cord, and likewise within the body of this monster, it cannot, our author thinks, be doubted that these communicated with each other, and that the blood was conveyed by them in a circle.

To describe the circle more exactly, we cannot, he adds, doubt that the blood was conveyed from the placenta by the umbilical vein into the body of the monster. We next found, he observes, that the umbilical vein within the monster was divided into various branches, which could be traced to all its parts, or that these branches performed the office of arteries,

or resembled the vena porta hepatica; and that contiguous to these branches were found, every where, other vessels forming a trunk or large vessel, which, by its situation, resembled our aorta: but we must suppose, continues our author, that these branches served the purpose of receiving the blood from the extremities of the branches of the umbilical vein, or were in reality venous vessels. From the vessel resembling the aorta in situation, but very different in office, two vessels were sent off, which ran at the sides of the bladder to the umbilicus, and formed the arteries of the umbilical cord and of the placenta; and, in the placenta, must have terminated in the minute beginnings of the umbilical vein, to complete the circle in which the foetal blood was moved.

Thus, he adds, we find the umbilical vein in the placenta and umbilical cord performing the office of a vein, while its continuation within the body of the monster performed the office of an artery; and, on the other hand, we see the vessel he has called aorta, performing the office of a vein within the monster, and that of an artery in the umbilical cord and placenta.

Dr. Monro next considers the causes of the motion of the blood in this monster. In that,  
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he observes, which was examined by Winslow, and which appears to have agreed very nearly with the one here described; no red blood was found in any of the vessels; and therefore we must conclude, that none of the red arteries of the mother anastomosed with the umbilical veins; and even where this is the ordinary structure, it is so far, adds our author, from being certain that the vessels of the uterus, which contain red blood, anastomose with those of the umbilical cord, that the contrary is the most probable opinion. He therefore considers it as very improbable that the blood in the umbilical vein was pushed on by the heart of the mother; and even were we to admit that the arteries of the mother anastomosed with the umbilical veins, yet as their communications must have been very minute, and the momentum of the blood in them very much broken, we cannot, he thinks, conceive that it could have been sufficient to push the blood through the terminations of all the branches of the umbilical veins, in the several organs of its body, into the vessel we call aorta, and again from the aorta back to the placenta by the umbilical arteries, and through the minute branches of these



these to the veins of the mother, and beginnings of the umbilical veins.

Hence our author is led to conclude that the circulation of the blood in the placenta and body of the monster was carried on by a well-regulated muscular action of the blood vessels. In the *echinus esculentus* he has found in the mesentery, which is a principal part of it, two such large vessels without a heart, and which, as he observes, we can scarcely doubt, resembled our aorta and vena cava, and circulated its fluid; and in fishes\*, he adds, the blood which passes through the liver describes three circles, and in all other parts of the fish the blood describes two circles before it returns to the heart; which motion of it we must suppose to be chiefly owing to the muscular action of the vessels, as the force of the heart appears to be as much spent in the gills of the fish as in the lungs of a man.

From considering the manner and cause of the motion of the blood in this monster, and comparing with it the motion of the blood in fishes, and in the sea egg, our author is, by

\* See Monro on Fishes, p. 67. tab. xliii.

analogy, led to the following general conclusions :

1. That the arteries contribute much to the circulation of the blood in our bodies.

2. That it is probable that, in man, the veins likewise assist in circulation ; and, in particular, that there can be no doubt that the vena portarum, by its action, contributes much to the motion of the blood through our liver.

3. That for the like reasons, we may conclude, that arterious vessels, independently of the impulse of the heart, may act in such a manner, as to perform the secretion of liquors, to nourish the solids, and to add to their bulk ; and particularly, that the branches of the vena portarum change certain parts of the blood into bile.

We come now to the author's remarks on the nervous system of this monster.

As the spinal marrow, and pairs of nerves sent off from it, had nearly the usual size and structure, although the brain, cerebellum, and medulla oblongata were entirely wanting, he finds reason for calling in question the common doctrine of authors, which teaches, that the spinal marrow and nerves derive their origin from the brain and cerebellum, and are dependent upon  
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it as much as the ducts of the glands are upon the glands which send liquors into them.

As the several parts of the monster were furnished with nerves, and we have seen that its arteries and veins, by a well-regulated, varied, and complicated action, circulated the blood, we must, he contends, suppose that their muscular fibres were actuated by those nerves. We therefore, he observes, find in this monster not only the existence and common appearance of the spinal marrow and nerves connected with it, although the brain and cerebellum were wanting; but we learn from it that these, independently of the brain and cerebellum, may actuate the muscular fibres in the vessels of an animal; or, in other words, that nervous energy, or fluid, as it is commonly called, is not derived from the brain and cerebellum solely; but that the nerves also are capable of furnishing it: from all which, he thinks, it follows, that there is no more reason for believing that the nerves are derived from the brain, than that the latter is derived from the nerves; and, of course, that all the parts and branches of the nervous system appear to possess the general power or office of furnishing nervous energy.

With respect to the duration of the life of  
this

this monster, our author remarks, that as in man and similar animals, the direct or indirect influence of respiration seems necessary for the continuance of life, and as the lungs were wanting in this monster, it must be supposed, that it could have outlived the separation from the mother for a very short time only; and when we farther consider, that, by the ligature of the umbilical cord, a stop would be mechanically put to the circulation of its blood, it is evident, he adds, that its life must have terminated with its delivery.

Dr. Monro concludes this instructive paper with observing, that as this monster was provided with a distinct placenta and membranes, and its body surrounded with and protected by the liquor amnii; as no vestige appeared of the brain, cerebellum, organs of the senses, or other parts of the head; as nervous threads, proper to this monster, ascended from the upper end of the spinal marrow towards the upper parts of its body; as its system of circulating vessels was complete without a heart, and the manner of their branching different in many respects from the common structure: it must surely appear, to an unprejudiced person, absurd to suppose, with many eminent authors, that

that such monsters, when first produced, had the ordinary structure, and that this was afterwards altered by pressure and other accidents.

A similar observation, he thinks, may be extended not only to many other monsters in his possession, but (as he believes he might say) to almost all other monsters which have been described; particularly to two, of which he has given a description, illustrated with figures, in his valuable work on the Nervous System (Table VIII.\*\* and Tab. XII.). In one of them, a human monster, one heart supplied two heads and two trunks; in the other, a kitten, one heart, consisting of two auricles and two ventricles, sent off from its left ventricle one aorta, which supplied one head and two bodies.



XVII. *Description of an Instrument for performing the Operation of trepanning the Skull, with more Ease, Safety, and Expedition than with those now in general Use.* By Samuel Croker King, Esq. Member of the Royal College of Surgeons in Ireland, and M. R. I. A.—From the *Transactions of the Royal Irish Academy*, Vol. IV.

THE various accidents and diseases which have ever been the lot of the human frame, must have called loudly on mankind to exert themselves for the relief of their suffering fellow-creatures; from these incidental calamities, the different operations of surgery have taken their rise, for the performance of which instruments judiciously contrived and properly constructed were absolutely necessary, as well for the ease and safety of the patient, as for the dexterity of the operator; while such were wanting, danger to the one, and difficulty to the other, unquestionably must have existed.

The improvements from time to time made  
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in the mechanic arts have afforded to the modern practitioners of surgery a manifest advantage over those of earlier ages; so that I may venture to affirm; that by the construction and workmanship of the instruments (joined to the assistance the operator derives from a knowledge of anatomy) more lives amongst the objects of surgery are at this day saved than in times past in similar cases.

If we look into the works of the old writers on the science of surgery, and examine the instructions handed down to us by them, for the performance of the principal operations, with the descriptions they have given us of the instruments they employed, we cannot wonder if many of their patients as often perished by the ill success of their operations, as by the maladies they attempted to remedy or remove. Notwithstanding (as Mr. Pott\* justly observes) they were well acquainted with the necessity of operations; yet, wanting the assistance of the ingenious mechanic to form their instruments, their intentions of affording relief to their patients were frequently frustrated.

In the present century, though the mode of

\* Pott's Works, Vol. I. page 122.

performing most of the capital operations in surgery, and the instruments proper for each, have undergone very considerable improvements and alterations, yet there seems to be room left for pursuing this useful branch of the healing art somewhat farther, particularly in the operation of trepanning, or perforating the cranium, which has continued to be effected with instruments fabricated nearly in the same form for a series of years; and considering that this operation must have been often necessary, from the accidents which have never ceased to occur, it is a matter of some surprise that the inconveniences attending the two instruments now generally used, namely, the trepan and the trephine, so called, have not been in some measure removed. The only attempt that has fallen under my observation was by Mr. John Douglas, a figure of whose instrument may be seen in the *Edinburgh Medical Essays* \*, and thus described: “ Two plates  
 “ of brass, kept together by four pillars of  
 “ brass, with a handle moving a tooth-wheel,  
 “ which turns a pinion to which the socket  
 “ for receiving a common saw-head of a tre-

\* Vol. V. Table iv. Fig. 6. page 374.

“pan is fixed.” This description is followed by a remark, “That the saw will be turned “*more equally* with this instrument than with “the hand alone; but whether the rattling and “trembling which the wheels make are sufficient to counterbalance this advantage, I “shall not determine.” By this we may suppose it had never been introduced into practice. To the above objection might be added, the chance that a machine of the construction now described might be liable easily to be put out of order: though the ingenuity of the invention must be acknowledged, and no doubt Mr. Douglas was well satisfied that something to render the operation of trepanning more expeditious, safe, and easy, was wanting.

Every surgeon will admit, that both the trepan and trephine are attended with inconveniences; as a proof of this, I need only produce the testimonies of two of the latest authors, who have enriched the science of surgery by their writings, namely, Mr. Pott, of London, and Mr. Bell, of Edinburgh. The former\* having strongly recommended the trephine in

\* Pott's Works, Vol. I. Note in page 125.

preference to the trepan, and the latter \*, on the contrary, having given his decision in favour of the trepan. These instruments are too well known to require a minute description here. I shall only remark, that the trepan is composed of a circular saw called a crown, fixed in a handle, which is turned round like a joiner's brace, with a knob on the upper part, on which the left hand of the operator is rested, to keep the instrument steady, while the right is employed to turn the handle. The trephine has a saw or crown of the same sort, fixed in an immoveable handle, either of wood or iron, somewhat resembling the handle of a carpenter's auger, and is worked by turning it backward and forward with the right hand.

The inconveniences † of the trepan must be obvious to every experienced operator, who will often find a difficulty to get either himself

\* Bell's System of Surgery, Vol. III. page 78.

† Quia Chirurgus trepanum sæpe non recte tenet, nec orthogonallyter ponit super cranium, ex quo fit ut uno latere tangat meningem antequam perforaverit alterum, hoc autem ex eo sæpe contingere potest, quod oculus Chirurgi ex alto aspiciens non bene possit videre an trepanum recte stet, nec ne, nisi ab astante medico admoneatur. Thomas Fienus de Trepano, Tractat. 1, cap. 3. fol. 6.



or his patient into a commodious situation. To operate with it, he must be placed above the patient, especially if the perforation is to be made on or near the upper part of the cranium. The assistants must be uncommonly attentive to keep the patient's head very steady, for the least motion will throw the trepan out of its direction, which, from its length, it is liable to. The difficulty of keeping a patient quiet, (unruly from the effects of the accident, or impatient under an operation which perhaps with reluctance he has submitted to) is well known to every operator.

To remove those impediments, some operators, besides keeping one hand firmly pressed on the knob, place their forehead on it, as directed by Dionis\*, or their chin, as advised by Garengéot†; these situations, besides very much confining the operator, must preclude him from seeing the progress of the saw; and when the furrow has been made into the second table of the skull, and the resistance against

\* Dionis, Cours d'Operations de Chirurgie. 5<sup>me</sup>. edit. 8vo. Paris, 1765, p. 520.

† Traité des Operations de Chirurgie, Tom. III. p. 187. 188. 8vo. Paris, 1731.

the instrument is become feeble, the bone may give way, and the saw, by being suddenly pressed in, may injure the dura-mater and brain, and consequently death ensue.

To prevent accidents of this kind, the older surgeons guarded the saw with wings or shoulders, and Ambroise Paré \* tells us, he invented a ferula or ring, which he applied to the saw, with a screw to secure it from too suddenly entering through the skull; afterwards, when the wings, shoulders, and ferulæ were laid aside, the saws were made of a pyramidal form, and were serrated on the sides as well as at the edge.

Heister †, sufficiently aware of the danger attending the trepan, advises, that when the skull is sawed deep enough, which may be known by the circular piece being a little loose, a terebra or gimlet should be screwed into the hole made by the center pin, and by the help of an elevator the piece is to be taken out; if it is not loose enough to come away, a few more turns of the saw are to be made, and the

\* Johnson's Translation of the works of Ambrose Parey, Book 10, chap. 18, p. 245.

† Heister's Surgery, part. 2, chap. 41, page 361.

terebra applied again, to bring out the piece without hazarding the wounding the dura mater by too frequent applications of the saw. And in all authors who have treated on this operation, we may find abundant cautions against injuring the brain or its membranes, by a want of attention, when the skull is nearly perforated. These considerations, no doubt, determined Mr. Cheselden\*, Mr. Sharpe†, Mr. Pott‡, and most of the eminent surgeons in England, to give the preference to the trephine, as a more safe and handy instrument.

The trephine has, in respect of safety, some advantage over the trepan; the operator can with more readiness apply it to any part of the head; being shorter, it is more manageable; and as there is less occasion for pressure on it, there is consequently less hazard of its suddenly slipping in and injuring the brain or its meninges; but then it is tedious, and divides the bone very slowly; the saw does only scrape the bone, the pronation and supination of the wrist

\* Cheselden's Observations on Le Dran's Surgery, page 447.

† Sharpe on the Operations of Surgery. 8vo. 1769, p. 157.

‡ Pott's Works, Vol. I. page 123.

cannot give it quite half a circle, and it is impossible to keep the hand so exact in its movement, but the furrow or fulcus will be *very uneven*; in short, it is not only fatiguing to the operator, but tiresome to the patient, especially if more than one perforation is to be made, which sometimes happens, as may be seen in Dionis\*, when the operation of trepanning was repeated twelve times on a young woman who had fallen from a ladder, by Messrs. Marschal and Dionis, with their two sons. Mr. Gooch† recites a case where thirteen perforations were made on the skull of an old man, who recovered. Scultetus‡ has given a case in which he made seven perforations round a depression, to disengage the fractured piece; and a very eminent practitioner§ of this city (while I am writing this) informs me, that very lately he made three perforations with the trephine, which he assures me was attended with

\* Dionis, Cours d'Operations, page 522.

† Gooch's Cases, Vol. II. 8vo. 1767, plate 1. page 1.

‡ Scultetus, Armament, Chir. Obs. 5. 8vo. 1659, p. 198.

§ James Henthorn, Esq. Secretary to the Royal College of Surgeons, in Ireland.

very great fatigue. If necessary, many like instances might be produced.

Though sawing the bone does not amount to pain, yet no doubt while that work is going on a disagreeable sensation to the patient must be excited; therefore, the more expeditiously it can be finished, consistently with safety, the better.

If these disadvantages attend the use of the two modern instruments, what must have been the case in former days, when the terebra or screw, the drill, the chisel, and leaden mallet, to break off the uneven edges of a fracture, or to divide the spaces between the perforations made by the terebra, were in use? or the meningophylax, which was a flat piece of silver or copper, like a spatula, thrust between the skull and the brain, to defend it from receiving injury from the chisel, when struck by the mallet of lead? Is it not charitable to conclude, that many of the patients of those days were permitted to close their existence without the aid of gimlets, drills, mallets, or chisels?

Mr. Pott \*, in his Observations on Injuries of the Head, has given us a very particular

\* Pott's Works, Vol. I. Note in page 125.



account of the instruments used by the ancient surgeons. As his works are in the hands of every practitioner, I shall only observe, that he has rejected the trepan as an *unmanageable instrument*, “ liable to most of the hazard and “ inconvenience attending the terebræ and te- “ rebellæ of the ancients.” In his opinion he coincides with Mr. Sharpe, who, in his Treatise on Operations, gives the preference to the trephine in the following words: “ I have used “ the word trepan all along for the sake of be- “ ing understood; but the instrument I recom- “ mend is a trephine\* ;” the advantages of which he describes, in the reference to the plate, as deserving a preference to the trepan, which, he says, is the instrument used in all parts of Europe, except Great Britain. And Mr. Cheselden†, in his Observations on Le Dran’s Operations, recommends the trephine in private operations, but the trepan when expedition is necessary, as in a battle or sea engagement. The handle of the trephine he desires to be made so heavy, that the hand may have little more to do than to direct it; but

\* Sharpe’s Surgery, p. 157.

† Cheselden’s Observations on Le Dran, p. 447.

Mr. Pott, differing in this particular from him, advises\* the handle to be made of light wood.

The instrument delineated by Mr. Sharpe, and approved of by him, is an exact copy of that recommended by John Woodall, Surgeon to St. Bartholomew's Hospital in London, and Surgeon General of the East India Company, in the reign of King Charles the First, in a second part of a work of his, entitled, "The Surgeon's Mate, or Military and Domestique Surgery;" he gives a plate and a long description of an instrument of his own invention, which he calls a *trafine*, (a tribus finibus) it being made with a handle, that each extremity might serve the purpose of an elevator of a different form; thus combining three instruments in one. The edition of the work now before me was printed in the year 1639, but I find, from the preface, he had written it in the year 1626, when in Italy: and as his description nearly corresponds with the instruments recommended by Mr. Cheselden and Mr. Sharpe, it is probable, on his recommendation, it had before their time been admitted into practice. My

\* Pott's Works, Vol. I. Note in page 123.

late friend, the celebrated Dr. M'Bride \*, has borne testimony, that John Woodall was a man of knowledge, experience, and observation; and, as a proof, refers us to his accurate account of the symptoms of the sea scurvy, and his directions for the treatment of that dreadful disorder, so often fatal in long voyages. He has bestowed many pages in enumerating the advantages of his *trafine*, and the disadvantages of the *trepan*, which, with all its faults, must have been deemed an acquisition, when invented, to the science of surgery; at a time when the *terebra*, *terebella*, *abaptista*, *mediolus*, *meningophylax*, *mallets*, *chisels*, &c. were the instruments in common use, no doubt the *trepan* must have thrown all those into disrepute. Fallopius † (among others) has condemned the *mallet* and *chisel*, and has cautioned his readers against them, lest by-standers and the public should, if the patient died, attribute his death to the treatment of the operator.

\* M'Bride's Experimental Essays, Essay 4, p. 186, 209.

† Ideo nolite uti hujusmodi *scalpro*, cum *malleolis*, sed potius *manibus*, si *vulgus* vestras est ita ut nostras. Fallopii Opera. Folio, Francofurti, 1606. Tom. I. p. 579.

We find even the writers of times not so remote, cautious of recommending the operation of trepanning. Peter Lowe, one of the first English writers on surgery, who styles himself Doctor in the Faculty of Chyrurgerie at Paris, and ordinary Chyrurgion to the French King and Navarre, in his work called, “ A  
 “ Discourse of the whole Art of Chyrurgery,” printed in London, in the year 1634, treating on the operation of trepanning, has these remarkable words: “ There is great judgement  
 “ to be used in doing this operation, and few  
 “ there are found that doe it well; many I  
 “ have seen of very learned and expert men,  
 “ and heard of divers to my great joy and  
 “ comfort, among which Mr. George Baker,  
 “ some time Chyrurgion Ordinarie to that worthy Prince\* Queen Elizabeth, and now to  
 “ his most sacred Majestie; a man of great  
 “ learning and experience, most fortunate and  
 “ dexter in this operation, like as in all other  
 “ operations of Chyrurgery;” and concluding his chapter with this pious ejaculation, “ *God*  
 “ *increase the number of such in this kingdome,*” plainly indicates that, at the time he wrote,

\* So in the original, which is in black letter, p. 320.

the science of surgery had not arrived to so flourishing a state in Britain as it has in these days. And John Woodall, the inventor of the trafine, in a former part of his work desires the young practitioner not to proceed too hastily in the use of the trepan, “for,” says he, “many\* worthy artists there are at this day “living, which have performed great cures in “fractures of the cranium, and yet never “knew the worthy use of this instrument,” (the trepan); and then he tells us that, in eight years living in Germany, he could not find that the German surgeons used a trepan, though they both spoke and wrote about it.

The English translator † of the works of Ambroise Paré has given us a figure of the trafine, and has quoted for his authority Doctor Helkiah Crooke, a physician, who flourished in England in the reign of King Charles the First. Probably the Doctor took the hint from John Woodall. By the dates of their works they appear to have been cotemporaries. In an

\* Woodall's Military and Domestique Surgery, page 4.

† Johnson's Translation of the works of Ambrose Parey, book x. chap. 18, page 246.



appendix \* to his Microcosmographia, he gives the figures of three-and-fifty instruments of chirurgery; after that of the trepan, (which, with all the others, is copied from Ambroise Paré, as acknowledged in his title page) he gives a figure of another sort of trepan or tra-fine, as in general use then amongst the London chirurgeons; it exactly resembles that of John Woodall, and is thus described, as called the head trepan: “ The head of which is  
 “ made taper fashion, smaller at the teeth,  
 “ and greater upwards, with cutting edges  
 “ round about on the outside, to make way  
 “ for itself; the shank of the head entereth  
 “ into the socket of a straight stemme, and is  
 “ made fast into it with a screw; the handle is  
 “ made cross the top of the stemme, like the  
 “ handle of a gimlet, but larger, and both  
 “ fides made in the form of an elevatory; this,  
 “ he says, with a semi-circular motion of the  
 “ hand, performeth the operation with great  
 “ security, for the perforation being made *it*  
 “ *cannot slip in, to endanger the hurting the dura*  
 “ *mater, as the other may do.*”

\* Appendix to Crooke's Microcosmographia, chap. 17, page 25.

In tracing the progress of the two instruments now in use for perforating the skull, we find that the *trafine* was introduced into practice in England in the last century, and succeeded to the *trepan* in its improved state; and though for some years past the *trepine* has been generally used by the English surgeons, yet, as I mentioned before, Mr. Bell has given a decided preference to the *trepan*, and has, by his recommendation, endeavoured to recal it into practice. His words are, “ \* If the tre-  
 “ phine is employed, all the *pressure and force*  
 “ necessary for turning the instrument is ap-  
 “ plied by one hand of the operator; the saw  
 “ is made to cut by forming half a circle only,  
 “ or scarcely so much, and the perforation is  
 “ finished by moving the saw backward and  
 “ forward, till the whole thickness of the bone  
 “ is divided; but when the *trepan* is made use  
 “ of, the surgeon applies all the *pressure upon*  
 “ *the head of the instrument with one hand,*  
 “ while he turns the handle of it with the  
 “ other. Some operators indeed make the  
 “ pressure upon it with their forehead, or with  
 “ their chin, but it is more easily and more

\* Bell's System of Surgery, Vol. III. page 77.

“ equally

“ equally applied with one hand, than it possi-  
 “ bly can be in any other manner ; by the tre-  
 “ pan the saw is made to move always in the  
 “ same direction, by which it cuts more easily,  
 “ and performs the operation in one-third of the  
 “ time that is necessary with the trephine ; as it  
 “ often happens that several perforations are  
 “ necessary, and as the operation is of conse-  
 “ quence *fatiguing to the operator and distressing*  
 “ *to the patient, that method of operating ought*  
 “ *surely to be preferred which renders the opera-*  
 “ *tion more easy, provided it is at the same time*  
 “ *equally safe.*” After having given this deci-  
 sion in favour of the trepan, for expedition and  
 safety, he has taken notice, “ that some prac-  
 “ titioners, very sensible of these advantages  
 “ of the trepan, but dreading the risk of its  
 “ *passing too suddenly in upon the brain, com-*  
 “ *mence the operation with this instrument,*  
 “ and finish it with the trephine. This, he  
 “ says, is far preferable to the usual method of  
 “ performing the operation entirely with the  
 “ trephine ; but those who have fully experi-  
 “ rience the advantages of the trepan will  
 “ employ it for the whole operation.”

I have been particular in reciting this pas-  
 sage, as Mr. Bell (to whom the science of sur-  
 gery.

gery is much indebted) has so widely differed in his choice of the instruments for perforating the skull, from the opinion of Mr. Cheselden, Mr. Sharpe, and Mr. Pott; but if we examine candidly the objections of Mr. Bell to the trephine, (some of which are certainly well-founded) we shall find they may, with equal justice, be applied to the trepan. If force or pressure be necessary for the trephine, force and pressure must surely be necessary for the trepan; without a degree of pressure the instrument cannot be kept in its place; for when the center pin is removed, is not the part of the bone within the sulcus the center round which the saw must turn? And if some sort of pressure be not made on the knob, by the sustaining hand, the saw cannot be retained in the furrow, so as to cut equally, but will, with the least motion of the patient's head, be thrown out, by which the operation must be retarded. The instrument, by reason of its length (few of them being less than eleven inches from the knob to the teeth of the saw, many of them more) cannot be replaced very expeditiously; it must be admitted, that it works quicker and more equably than the trephine; but though the trphine invented by John Woodall was made

to cut in its motions backward and forward, (that is, I suppose, the teeth of the saw set alternately in opposite directions) yet notwithstanding this mechanism, that it is laborious and tedious every experienced surgeon will allow; no doubt this was the reason which induced many operators to begin the operation with the trepan for expedition, and finish it with the trephine for safety.

In the hands of a judicious and careful operator, who has had opportunities of frequently practising this operation, either of the instruments now in question might be safe, and I should be sorry to suppose that any fatal accidents have happened in these our days in consequence of using them without due attention. But when we consider that trepanning is an operation of the utmost necessity, and seldom will admit of delay; that the patient who has met the accident, and is to be relieved, is often too remote from such help as he or his friends would wish to apply to; that the nearest practitioner is to be called upon, who must go to work without the advantage of having the advice or assistance of an associate of the profession, which is not the case in general with most other surgical operations, where the patient



has time to consider and make choice of the person into whose hands he will commit himself, and leisure perhaps to remove to the place where the assistance he approves of is to be obtained; but injuries of the head will not at all times allow of such advantages.

When, I say, we consider all these circumstances, and reflect that so many of the most eminent and able operators have differed materially in the choice and form of the instruments for the purpose of perforating the cranium, and that the objections made to any one instrument were in some measure applicable to them all, we shall be led to think, that if an instrument could be devised, in which might be united the expeditious and equal working of the trepan, with the safety of the trephine, a valuable addition thereby would be made to the manual part of surgery: It will be readily granted, that in every profession all are not alike expert in using their hands; this may be observed in every mechanical trade or occupation; but if we can render the instruments so simple in their structure, that all difficulty in working will be removed, we shall thereby bring the performers more on an equality; and as in the operation we have been treating of, a

cautious attention to avoid injuring the brain or its membranes is so requisite, we cannot be too seriously on our guard. Though the business of trepanning, simply considered, is no more than sawing a portion of the skull, yet that business, injudiciously or incautiously executed, may be the cause of putting an end to the life intended to be saved, or as John Woodall expresses it, \* “ A man may in a moment be slain by art, for want of art.”

In using either the trepan or trephine, force or pressure, for obvious reasons, should, as much as possible, be avoided. We may observe that a carpenter, in sawing, knows, that if he applies too much weight on his saw he will retard its progress; therefore he depends more on the dexterous manner of handling the saw than on his strength or the weight of his hand; and in these operations of surgery, which are merely mechanical, we should not disdain to take instructions from the performance of those artists from whom we have condescended, in some measure, to borrow the fashion of our instruments. The truth of what I have advanced every surgeon must have perceived

\* Page 317.

when sawing through the bone in amputating, and a little experience will teach him, that he will sooner accomplish the work by a proper adroitness than by dint of force or strength.

The instrument which I have the honour of laying before the Academy, I am inclined to think, will render that necessary operation of trepanning more easy, more expeditious, and more safe. If I have succeeded in this attempt, I shall esteem myself fortunate to have contributed to the improvement of so useful an art as that of surgery; but if I have done no more, I have offered another instrument, to those already in use, for the operator to choose from, in an operation which requires to be properly and cautiously performed, and is frequently the chief, sometimes the *only relief* for the person who has the misfortune to be the subject of it. In using this instrument, neither force nor pressure is requisite; the left hand, which is employed to sustain the instrument, by being placed near the saw or crown, will keep it steady and firm to the place intended to be perforated, while the right hand is engaged in turning the handle faster or slower, as the operator shall judge expedient; the points on which the saw turns being at each end of the

canula, and not resting on the two extremities of the instrument, (which is one of the faults of the trepan) will require no more force or pressure on it, while working, than what may be sufficient to keep the teeth of the saw in contact with the bone; consequently, (pressure not being necessary) all hazard of suddenly entering or wounding the dura-mater or the brain is prevented.

This instrument is composed of a crown or saw made in the usual form, and about an inch and an half from the crown is fixed to a spindle four inches and an half long, which is received into a barrel or canula of four inches in length; to the top of the spindle, which is square above the canula, is applied a handle or winch, with a nut screwed on the spindle to keep the handle on; at the lower part of the canula or barrel is a flat rim, projecting about a quarter of an inch, on which the left hand, which grasps the canula, rests, to prevent it from slipping down on the part of the instrument below it, which is turned by the handle above; the crown has a center-pin, as in the other instruments, with a key to remove it when the sulcus is deep enough to admit it to be taken away. Though with this the operation may with great safety

ease and safety be entirely completed, yet to accommodate those who wish to finish with the trephine, the upper part of the barrel or canula is made square to fit into a wooden handle; upon applying this handle, instead of the winch, the instrument is converted into a trephine; in this wooden handle is a square opening, fitted to the square part of the spindle, and fastened by the same nut.

It will be requisite to have two or three crowns of different sizes, that the operator may choose that which will best suit the circumstances of the case; as to the form of the crowns, modern practitioners have so differed about them, that every surgeon must be left to his own choice. For my part, I should prefer those as the best and safest which are neither too conical nor too cylindrical, but between both extremes; and, as Mr. Cheselden\* has advised, the cavity in the inside to enlarge in the same manner the outside does, to prevent the piece of bone to be taken out from being wedged in the cavity, and to allow the crown or saw to be inclined to one side or to the other, as occasion may require, during the operation.

\* Cheselden's Observations on Le Dran, page 447.

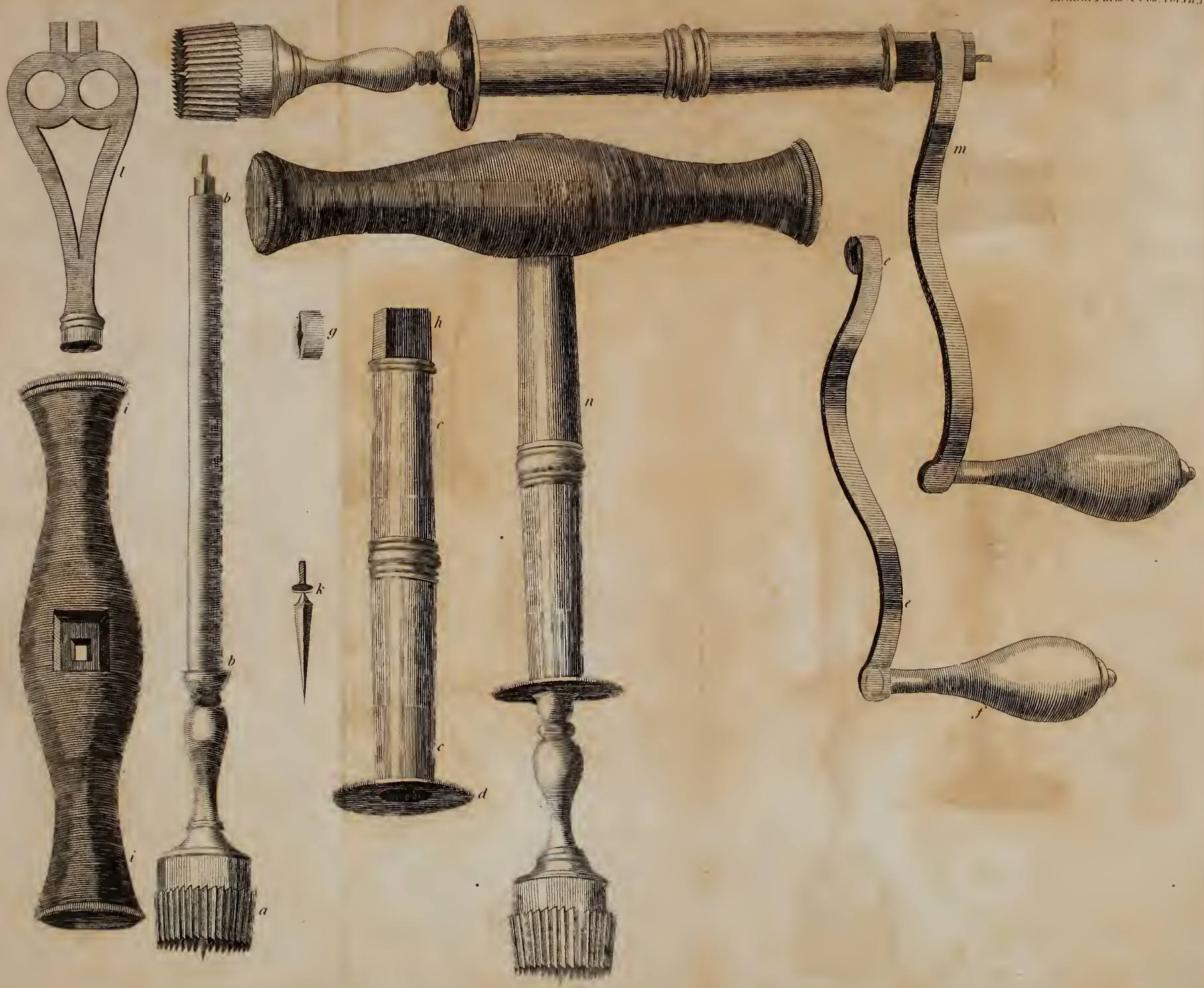


That the form of this instrument may be better understood, I have annexed engraved figures \*, not only of the parts of it separately; but likewise of all these parts together, when to be used.---I am well aware, that in offering this instrument to the public, I may have to combat not only with the prejudice of a great many years, but with some of the most respectable authorities in the profession, who have been long in the habit of using the other instruments with success; but from the experience I have had of it myself, and from the approbation it has met with from many eminent surgeons in this city. I am tempted to send it into the world, with a hope that no practitioner will condemn it without first having given it a fair trial; if, then, any alteration should be suggested, to render it more safe, useful, and convenient, the improvement shall be thankfully adopted.

I must observe, that in the foregoing pages the reader will find the instrument called trephine sometimes written *trafine*. In this I have followed the spelling of the authors I have consulted. *Trafine*, a *tribus finibus*, was the name

\* See Plate III.





it received from the inventor, John Woodall; it has since, for what reason I know not, been changed to trephine; but the alteration in the letters of the word (the pronunciation being the same) is immaterial.

*Explanation of the Plate\*.*

- a* The crown or saw.
- bb* The spindle to which the saw is fastened.  
Note, the spindle is to be made smaller from each end to the middle, that it may not touch the barrel but at the upper and lower parts, to prevent too much friction in turning.
- cc* The canula or barrel into which the spindle is introduced.
- d* The lower part of the canula, which projects something above a quarter of an inch, on which the left hand is rested.
- ee* The handle, with a square opening to fit the end of the spindle.

\* The drawings for this plate were taken by Mr. John Ellis, from the first-made instrument, which was executed with great exactness, from my verbal instructions; by Mr. John Read, an eminent cutler and instrument maker, of Skinner Row, in this city.

*f* The



- f* The knob of the handle, which should be made of ivory.
- g* The nut to screw on the end of the spindle to keep the handle on.
- h* The end of the canula, formed square to fit the wooden handle when used as a trephine.
- ii* The wooden handle fitted to receive the canula and spindle; when used as a trephine, the upper side is counter-sunk to admit the nut.
- k* The center-pin of the crown or saw.
- l* The key, one end of which is made to fit the center-pin, and the other, which is forked, to fit the cavity in the top of the nut.
- m* A view of the instrument, with the several parts put together when used as a trepan.
- n* A view of the instrument when intended for a trephine.



XVIII. *Case of an enlarged Spleen.* By George Burrowes, M.D. M.R.I.A.—Vide *Transactions of the Royal Irish Academy*, Vol. IV.

THE subject of this case was a man, aged about forty-four years, who, in 1792, was admitted under the author's care at the Hospital of the House of Industry, in Dublin.

The patient said he had laboured under dropsy for some months, during which time the abdomen had increased to the size it then was, which was equal, we are told, to that of a woman in the ninth month of pregnancy. His eyes were suffused with yellow, but his legs were not then, nor had they ever been, swelled. The abdomen was examined by some of the pupils at the Hospital, who thought they could perceive an evident fluctuation.

On the day after his admission (April the 5th) he took an opening medicine, which operated well; but on the following day (the 6th) he was seized with severe vomiting and head-ach, which prevented any examination of the swelling, and was ascribed to fatigue, (as he had walked several miles from the country to the Hospital) change of air, diet, &c.

The

The vomiting continuing on the 7th, the effervescent draughts were prescribed him, and some wine. On the 8th, his pulse was extremely quick and feeble, his skin hot, and tongue white, and he had raved at times during the night. Blisters were then applied to the legs, an injection ordered, and the wine increased. The vomiting still returned at intervals. On the 9th, all the symptoms became much more violent, and slight hiccough disturbed him now and then. The wine was again increased, being the only thing his stomach would bear. On the 10th, he was in a state nearly approaching to coma, with constant singultus. Attempts were made to rouse him by farther stimulating applications, but in vain; and he died that evening, the sixth from his admission, and the fourth from the attack of vomiting.

Our author examined the body the morning after his decease. From the circumscribed appearance of the tumefied abdomen, he supposed the water might be contained in a cyst, (a circumstance, he observes, seldom met with in men) as it much resembled ovarium dropsy, a disease very familiar to him in the Hospital. On striking the tumour on one side, while he

kept

kept his hand fixed on the other, he had no doubt of feeling a fluctuation; but when he pressed hard on the linea alba, or center of the abdomen, he was greatly surprised, he tells us, to find a ridge reaching from the sternum to the pelvis. He then opened the cavity, and beheld the stomach and intestines all thrown completely to the right side, and much distended with air, while the whole left side was filled up with the spleen, enlarged to an enormous size, and occupying the entire half of the cavity, from the diaphragm, which projected into the thorax, to the pelvis; it was neither discoloured nor indurated, it seems, but natural in every respect, except size; and the vessels were not more than a third larger than in an ordinary sized spleen.

When this enormous viscus was taken out, it measured, we are told, fourteen inches and a half in length, and weighed eleven pounds thirteen ounces\*. The liver was somewhat discoloured and diseased, but not a spoonful of water was contained in the abdomen.

The evenness and soft texture of the viscus

\* The usual length is about four inches, and the weight six or seven ounces.

on one side, and the equability of the other from the distended intestines, joined to the facility with which such bodies would convey the *iectus*, when struck on the *left* side, might, the author observes, have deceived even an experienced surgeon; and had a person with such a disease been tapped, the trocar must have inevitably been plunged into the viscus, and death (from hæmorrhage) have ensued. Such a circumstance, he adds, did once happen in Edinburgh, and the wounded spleen is exhibited by Dr. Monro in his lectures\*.

The stomach was changed in position as well as site, lying more obliquely than horizontally; and so compressed was it between the liver and this enlarged spleen, that it required, we are told, very considerable force to distend it. That part of the colon which is tied down by a ligament, had, it seems, in consequence of the great pressure, its diameter much lessened, and the thickness of its coats was very considerably increased.

It is by no means uncommon, Dr. Burrowes

\* An account, by Mr. Ford, of a similar accident, in a case where the spleen, after death, was found to weigh upwards of four pounds, may be seen in Medical Communications, Vol. II. page 137. 8vo. London, 1790. EDITOR.

remarks,

remarks, to find this viscus preternaturally enlarged; and instances of this sort are related by various authors. Mead mentions the dissection of a person in whom was found a spleen weighing five pounds; and Morgagni informs us of his having seen one of eight pounds. But that which is the subject of the present paper, seems to be larger than any before described.

Our ignorance of the uses of this viscus is considered as one of the disgraces of physiology. Much time, our author observes, has been employed, and much ingenuity exhausted, by the ancients as well as the moderns, in endeavouring to ascertain its purposes in the animal economy, but without success. Malpighi, he remarks, dissatisfied with the conjectures of his predecessors, and despairing of succeeding where so many had failed, resolved on an experiment from which he might rationally have expected considerable information. He extirpated the spleen in several dogs, hoping thus to discover by its want what had in vain been sought for from its presence. His ingenuity was, however, but ill required; for though the animals lived long after the operation, and constant attention was paid to every circumstance attending them, and the experiment has  
 been



been repeated in England by Mr. Boyle and others, yet it has not in any wise assisted us to discover what are the secretions of this gland, or what its use. The opinion which the ingenious Mr. Hewson advanced a few years ago, seems to our author as wild as the conjectures of those who preceded him; but although the experiment of Malpighi has not discovered to us the use of this viscus, it has, however, Dr. Burrowes observes, proved to us, that it is not *indispensably* necessary to animal life; and the case which he has related, he thinks evinces that it may be increased immensely, without affecting the constitution otherwise than by mechanical means. “Enlarged enormously as it  
 “ was,” says he, “it did not prevent fever,  
 “ nor did it seem to give rise to any disease.  
 “ Dropsy, the common consequence of ob-  
 “ structed or even enlarged viscera, was not  
 “ produced by it; and had not the fever acci-  
 “ dentally come on, the man might have lived  
 “ till the pressure had prevented digestion from  
 “ being performed.” That it aggravated the symptoms of the fever, and contributed to its danger, he will, he adds, readily confess; but he by no means thinks that it can be considered as a *cause* either exciting or remote.

XIX. *An Estimate of the Excess of the Heat and Cold of the American Atmosphere beyond the European, in the same Parallel of Latitude: to which are added, some Thoughts on the Causes of this Excess.* By Edward Aug. Holyoke, M. D. F. A. A.—From the *Memoirs of the American Academy of Arts and Sciences*, Vol. II. Part I. 4to. Boston, 1793.

## PART I.

ALMOST from the first discovery of North America, it has been observed, that the extremes of heat and cold are much greater on this side the Atlantic ocean, than they are in Europe, under the same parallel of latitude. But the quantity of this difference has not hitherto, so far as I am acquainted, been an object of much attention, or been determined with any degree of exactness. A valuable work, published, for some years past, by a Meteorological Society at Manheim, in Germany, entitled *Ephemerides Meteorologicæ Palatinate*,  
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*time*, affords data for determining this point more precisely, as it contains more numerous and more accurate observations than any other publication extant.

I have therefore, from this collection, formed a table of the greatest heat and greatest cold, and of the mean of the greatest heat and cold, for a course of years, of twenty different cities in Europe; the southernmost of which is Rome, in lat.  $41^{\circ} 53'$ , a few minutes southward of Boston; and the northernmost, Stockholm, the capital of Sweden, in lat.  $59^{\circ} 20'$ , comprehending an extent of upwards of  $17^{\circ}$  of latitude; and from Rochelle, on the western coast of France, to Buda, the capital of Hungary, comprehending  $20^{\circ}$  of longitude; which takes in all the middle region of Europe. To which are added, my own observations of the greatest heat and cold, &c. made at Salem, in Massachusetts.

By this table it appears, that of the twenty European cities mentioned in it, the thermometer was highest at Wartzburgh, in the circle of Franconia, viz.  $102^{\circ}. 4$ , which falls short of our greatest heat above 3 degrees. The greatest degree of cold happened at Sagan, a city in the western borders of Silesia. There  
the

the mercury in the thermometer sunk to— $21^{\circ}.32$ , which exceeds our greatest cold at Salem by  $10^{\circ}.3$ ; but is just as low, as we were informed by the public prints at the time, though I know not upon what authority, that the thermometer fell at Hartford, in Connecticut, and at New York, in the month of January, 1786. But what is most to our purpose, the *mean* of the greatest heat in all those places, taken collectively, for the period noted in the third column of the table, amounted to no more than  $+86^{\circ}.41$ , which is more than  $10^{\circ}$  short of our greatest heat at Salem: and the mean of the greatest cold in these twenty cities, amounted to  $+3^{\circ}.31$ , which is short of the mean of our greatest cold upwards of 5 degrees.

But in order to determine the difference between our heat and cold, and the European, in the same latitude, we must compare with those cities, which are situated in latitudes nearest our own, viz. Padua, Marseilles, and Rome. We find by the table, that the mean of their greatest heat falls short of ours  $5^{\circ}.62$ ,  $7^{\circ}.42$ , and  $11^{\circ}.59$ , respectively. We also find the mean of the greatest cold of these three cities is less than ours by  $19^{\circ}.41$ ,  $29^{\circ}.92$ , and  $35^{\circ}.88$ , respectively. Further, the mean of the great-

est heat of these three cities, taken collectively, which is  $88^{\circ}.1$ , deducted from the mean of our greatest heat, which is  $97^{\circ}.02$ , leaves a difference of  $8^{\circ}.92$  hotter: And the mean of the greatest cold of these cities, being  $+25^{\circ}.96$ , taken from the mean of our greatest cold,  $-2^{\circ}.42$ , gives a difference of  $28^{\circ}.38$  colder.

The air of America then, in our latitude, is hotter in summer (when hottest) by 10 degrees of Fahrenheit's thermometer\*, and colder in winter (when coldest) by 5 degrees, than the whole middle region of Europe taken collectively, whose mean latitude is about  $49^{\circ}$  or  $50^{\circ}$ , that is, about 7 or 8 degrees more northerly than Boston.

Again, the air in America is hotter in summer, by upwards of 8 degrees, and colder in winter, by 28 degrees, than those parts of Europe, which lie nearly in the same latitude†.

\* I have all along made use of Fahrenheit's scale, as being much more familiar to us than Reaumur's.

† As these three cities, taken together, lie a degree or two northward of us, the result here given is rather less, than the true.



## PART II.

Here then is a very notable difference in respect both of heat and cold, in two tracts of our globe, which equally enjoy the influence of the sun, that prime source of heat to our system; and it is much greater, I believe, than any one would imagine, who had not attended to observations of this kind: which naturally leads to an inquiry into the cause of so remarkable an excess; for this cause still remains a problem, which has never, I suppose, received a satisfactory solution.

Various conjectures have been formed upon the subject; one or two of which I will mention. The first, and, perhaps, the most commonly received opinion among us, is, “ That  
 “ those lakes and large tracts of inland waters,  
 “ which lie back of our settlements, being  
 “ constantly frozen over every winter, expose  
 “ a large surface of ice to the air; which be-  
 “ ing hereby rendered very cold, and being  
 “ soon wafted to the sea-coast, where our most  
 “ numerous settlements are situated, occasions  
 “ that degree of cold, which the inhabitants

“ of North America suffer beyond the Europeans  
 “ in the same climate.” To which I object,  
 1st, That the winds, which for the most part  
 produce our most intense cold, are not westerly,  
 as upon this hypothesis they ought to be, but  
 north westerly, or still more northerly winds,  
 which last certainly do not blow over any great  
 extent of water in their passage to our coasts.  
 2d, The cause assigned does not seem adequate  
 to the effect; for though the lakes to the west-  
 ward of us are large, yet they bear but a small  
 proportion to the extent of land over which  
 the winds must pass, ere they arrive at our set-  
 tlements. 3d, I suppose that a surface of frozen  
 water does not render the air, that passes over  
 it, in any degree colder, than an equal surface  
 of frozen earth; for frozen earth is as cold as  
 frozen water; and all the surface of the ground,  
 between the lakes and us, is frozen every  
 winter, before the lakes themselves are. And,  
 if they do not render the air colder, after they  
 are frozen, than an equal surface of frozen  
 earth; certainly it cannot be supposed that they  
 increase the cold, before that period. Add to  
 this, 4th, That this surface of ice, which co-  
 vers the lakes every winter, is pretty early in  
 the season clothed with snow, more or less

deep, as well as the whole surface of these northern countries: now, no one can suppose that snow, because it lies upon a surface of water or ice, is capable of producing a greater degree of cold in the atmosphere, than if it covered an equal extent of ground. These observations do, I think, evidently show, that this hypothesis is not admissible.

Others have supposed, “ that our woods and  
 “ thick forests, by harbouring large quanti-  
 “ ties of snow every winter, and screening it  
 “ from the action of the sun’s rays, do occa-  
 “ sion the air, which blows over it, to be much  
 “ colder than it would be otherwise, or than  
 “ it is in Europe.” This may, indeed, in some measure, account for the length of our winters, and the sharpness of the winds in the spring; as the snow will remain longer undissolved, when shaded from the sun, than in the open and cultivated parts of the country: but I do not conceive how the snow, which lies in the woods, should communicate any extraordinary keenness to the air, beyond that which covers the ground every where in these regions, whether cleared or uncleared, during the winter season.

However, that the woods of America are

somehow the occasion of its greater cold, must, I believe, be admitted.

Several writers have observed, that the winter's cold in the old continent was formerly much more severe and intense in the same climate and the same spot, than it is at this day. This is a remarkable fact, of which, however, I imagine there is sufficient proof. For severe frosts are mentioned by ancient authors as common events, in particular places, where nothing of the kind occurs now; or, if at some distant intervals they do still sometimes happen, they are constantly noted as very extraordinary.

David, King of Israel, in one of his psalms\*, says, "He giveth snow like wool, he scattereth the hoar frost like ashes. He casteth forth his ice like morsels; who can stand before his cold?" And about the time of our Saviour's crucifixion (about the beginning of April) St. John tells us†, that "the servants and officers had made a fire of coals (for it was cold) and they warmed themselves."

And Juvenal makes mention of the freezing of the river Tiber, as a common event in his

\* Psalm cxlvii. v. 16, 17. It is not material whether David was the author of this psalm, or not.

† John, ch. xviii. v. 18.

time\*. And Ovid talks of frozen wine in countries, where, I suppose, very severe frosts are now very unusual†.

If any doubt respecting this point should still remain, I believe it will be much lessened by an attention to what Livy, the Roman historian, relates, in his account of the second Punic war. There we find, that when the Romans, under the command of Scipio, besieged a town in Spain, near the river Ebro, in a latitude a little more southward than ours, he says, “ *Nec obsessos alia ulla res, quàm iniqua oppug-*  
“ *nantibus hyems tutabatur. Triginta dies obsi-*  
“ *dio fuit; per quos raro unquam nix minus qua-*  
“ *tuor pedes alta jacuit: adeoque pluteos ac vi-*  
“ *neas Romanorum operuerat, ut ea sola, ignibus*  
“ *aliquoties conjēctis ab hoste, etiam tutamentum*  
“ *fuerit.*” (Lib. xxi.) That snow should lie four feet deep on the ground for thirty days together at Taragona or Barcelona, (in the neighbourhood of which this town lay) would, at the present day, be looked on as a most extraordi-

\* Hybernum fracta glacie descendit in amnem, &c. Sat. vi. l. 521.

† Nudaque consistunt formam servantia testæ

Vina: nec hausta meri, sed data frustra bibunt. *Trist. Lib.*  
3. *Eleg. x.*



nary phenomenon indeed. See also Virgil's 3d Georgic.

And, as no change has taken place upon the surface of the earth in that continent, that we are acquainted with, so remarkable, and so likely to have any great influence upon the atmosphere, as that of cutting down and clearing the earth's surface of those woods and thick forests that abounded every where; may we not probably conjecture that this circumstance is somehow the cause, why it is warmer in Palestine now, than in the days of King David; and at Rome, than it was in the times of the commonwealth, or of the Cæsars \* ?

Now, it appears highly probable, that the same cause, whatever it was, which rendered Europe colder formerly than at present, makes America at this day colder than Europe. America is at this day in a situation similar to that

\* It appears by the annexed table, that at Rome there does not happen every year, at this day, such a degree of cold, as to sink the thermometer down to the freezing point. The difference then between its atmosphere in Juvenal's days, and the present, must be very great. And, as to Jerusalem, as it lies about ten degrees farther south than Rome, I presume that nothing like a frost ever happens there, at this day.

which

which Europe was in, with respect to its woods, thirty, or perhaps thirty-five, centuries ago. Its surface, excepting about a hundred or a hundred and fifty miles, more or less, along the sea coast, is almost universally covered with thick and almost impenetrable forests, as is well known to every one. And, as the same causes always produce the same effects, it seems very probable, that the forests of America are, in some way or other, instrumental in producing that extra degree of cold, for which our winters are so remarkable.

Taking this, therefore, for a probable supposition, let us pursue it, and inquire whether it be confirmed by reason and experiment.

Among the many happy discoveries in philosophy and chemistry, with which the celebrated Dr. Priestley has obliged the world, one of very great importance is, “ The property,  
 “ which the leaves of all plants and vegeta-  
 “ bles of every kind possess, of yielding, in  
 “ day-light, air of a much purer kind, freer  
 “ from phlogiston, and fitter for respiration,  
 “ than common atmospheric air : that they not  
 “ only furnish large quantities of such air, but  
 “ have also the faculty of absorbing phlogiston  
 “ from air, when fouled by a mixture of it,  
 “ so

“ so as to render the same salubrious and respi-  
 “ rable, which was before noxious and suffo-  
 “ cating; and thus become, in the hands of  
 “ the great Author of Nature, one grand cor-  
 “ rector of those impurities which might other-  
 “ wise so far increase, as to contaminate the  
 “ whole mass of the atmosphere; and in pro-  
 “ cesses of time render it totally unfit for respi-  
 “ ration, and the support of animal life.”  
 This is a doctrine well established, and needs  
 no new proofs.

All vegetables, then, both in Europe and  
 America, are continually supplying the atmos-  
 phere with this pure air, and counteracting  
 those phlogisticating processes, such as com-  
 bustion, respiration, putrefaction, &c. which  
 are continually going on in all parts. But there  
 is this material difference between the two con-  
 tinents: in Europe, upon the coming on of  
 the frosty season, the leaves of all vegetables,  
 on or near the earth's surface, languish; and,  
 if they do not die, yet most probably they per-  
 form their office of dephlogisticating the air,  
 in a much more languid manner, than in sum-  
 mer; or are perhaps entirely covered with  
 snow, which, while it continues on them, must

effectually put a stop to this process; and as to the trees, their leaves for the most part drop off, and no more pure air is to be expected from them, till they are again renewed in the spring. But in America, although the leaves of all vegetables on the earth's surface are frozen and killed early in the winter, and the leaves of many of our trees fall off, and yield no more of this pure air than the European trees, yet there is a constant and large supply of it, from those vast quantities of pine trees, firs, spruce, cedars, junipers, fawns, hemlocks, and other ever-greens, which retain their leaves through the intensest frosts, and which do greatly abound in our American woods, from the 30th to the 50th degree of N. latitude; a quantity sufficient, perhaps, to cover one 5th or 6th of the whole surface of the continent of Europe.

That our evergreens do in fact yield, during the winter season, such a pure air, I have several times found by experiment. My experiments were conducted in the manner in which Drs. Priestley and Ingenhousz conducted theirs; and I constantly found air produced from the leaves of juniper and pine (the only ones I have yet made trial of) in the same manner as from

from other leaves in summer \*. But it ought to be observed, that this manner of experimenting cannot be prosecuted, when the water is colder than 32 degrees by Fahrenheit's thermometer, as the water would then be converted into ice; but it appears reasonable to suppose, if as much air is produced in these experiments, when the thermometer is at 34° or 35°, as when it mounts to 70° or 80°, that the weather, though much colder, would make no material alteration in the result. This fact then is not to be considered as fully established (though, I believe, whenever proper and decisive experiments are made, it will be confirmed) but as in a good degree probable.

This being allowed, what a fund of pure de-

\* I have not yet had either time or opportunity to prosecute experiments upon evergreens as I could wish, or as I promise myself I may have; but, from what I have experienced, I think it no extravagant supposition, that a pine tree, of a common size, should yield four barrels of dephlogisticated air in one clear fair day; to which, if we add, that as much phlogiston is absorbed in the same time as an equal quantity of atmospheric air contains, we may readily imagine, that, in a country abounding with trees of this kind, the atmosphere must be much more dephlogisticated in winter, than in a country where evergreens are rarely met with.

phlogisti-



phlogisticated air have we here in America, beyond what Europe at present possesses. There are no doubt evergreens of various kinds scattered all over Europe; yet it cannot be supposed, that the quantities bear any proportion to those which once flourished there; especially in the southern and middle regions of that continent; and as to Scandinavia, where I suppose they abound most at present, I imagine they must be very much thinned by this time. But what will all these amount to, when brought into a comparison with the evergreens of America?

From these considerations, I think it must appear highly probable that America is furnished with sources of dephlogisticated air, which are now exhausted in Europe; and that therefore, most probably, its atmosphere is really more pure and dephlogisticated.

Whether this be in reality the fact, may be determined most satisfactorily from meteorological observations. For the sensible qualities of the atmosphere, which are the objects of meteorology, may, if properly attended to, and noted down for a course of years, determine not only which country enjoys the driest and purest air, but also the quantity of the difference

difference (if any there be) may as easily and as precisely be known, as the difference of their heat and cold.

To such observations then we must recur; and if we are enabled to determine the quantity of evaporation, the quantity of rain, the number of clear fair days, the number of cloudy days, of rainy days, and of foggy days, in each continent, for a competent course of years, there is no doubt but the point may be satisfactorily determined. This I shall next attempt; after premising that we have not yet, perhaps, observations enough to settle the matter without all doubt, yet enough, I imagine, to show that it is in the highest degree probable, that the climate of America is much dryer in general, than that of Europe\*.

The quantity of evaporation in any country must, I think, depend principally, if not entirely, upon the three circumstances of dryness, heat, and motion of the air, contiguous to the

\* Perhaps the most direct way of determining the dryness of the atmosphere is by the hygrometer; but till this instrument is more improved than at present, and observations have been made upon it for some competent time, in both continents, this mode of determining it must remain a desideratum.

evaporating surface\*. For the dryer the air, the more capable it is of absorbing a certain quantity of water in a given time; for when fully saturated with water, as in a foggy season, little or no evaporation takes place. Heat too is found to promote evaporation, probably as it lessens the cohesion of the particles of water: and the wind, not only by agitating the evaporating surface, but also by applying fresh portions of air to the same, tends greatly to promote this process.

Now all these circumstances conspire with us in America, in a greater degree than in Europe, to increase the dryness upon the surface of the earth. And such a degree of dryness does in fact take place here, as much more frequently to injure our crops, and frustrate the hopes of the husbandman, than in Europe.

The proof of this point, however, from actual observation, according to the *Ephemerides Meteorologicæ*, is rather lame; for of the six or seven places mentioned in that work, which can be easily brought into a comparison with

\* Electricity may perhaps be considered as another cause promotive of evaporation; but then I suppose it probable, that its effect in promoting evaporation may be very much in proportion to the dryness of the atmosphere.

those of Dr. Williams in the same work, which are the only American ones that I have met with, two, if I understand them, exceed his considerably: all the rest indeed fall much short; for the mean evaporation of those seven places does not amount to quite 45 inches for the year 1785; whereas the evaporation at Cambridge the same year, by Dr. Williams's account, was upwards of 56 inches.

The great difference in the quantity of rain which falls, in different countries, annually, makes this a remarkable article in the meteorological register. We are informed by Dr. Lind\*, that at Senegal, in Africa, there falls, in the four rainy months, 115 inches depth of rain; and by the *Ephemerides* so often quoted, that at St. Petersburg, in Russia, in the year 1785, there fell short of 12 inches†. Now I think it almost certain, that the quantity of rain that falls yearly in any country, provided it be sufficient for the purposes of vegetation, must be very much in proportion to the annual quantity of evaporation in the same region;

\* Diseases of hot climates.

† And we are not informed that this year was remarkably dry there.



for a very trifling quantity would any where be enough, if none of it were to pass off by evaporation; as, on the other hand, scarcely any assignable quantity would be sufficient, if the whole of it were suddenly exhaled. The dryer the air is, then, in any large extent of country, the more rain is required to support vegetation in its full vigour: so that the comparative dryness of the atmosphere, in any two countries, may be pretty fairly inferred from the annual quantity of rain which falls in each of those countries respectively, for a course of years, provided vegetation be equally vigorous in both.

From the many registers which have been published of the depth of rain, which falls in a great number of places in Europe, and for a long course of years, it appears, that the medium quantity of rain in that quarter of the globe scarcely equals, but certainly does not exceed, 30 inches from year to year. But in America, viz. at Ipswich-Hamlet, by the observations of the Rev. Mr. Cutler, upon a mean of five years, (the last of which, viz. 1787, was rather a dry one) there fell inches 49. 472. And by Dr. Williams's observations at Cambridge, there fell, in 1785, inches 47. 616. And by the observations of the Rev. Mr.



French, at Andover, there fell there, on a mean of the seven last years, inches 51. 2. annually.

The number of fair unclouded days which happen in the course of a year, for several years together, in any place, must also give some indication of the dryness of the atmosphere of that country; for as clouds are formed from the moisture existing in the air, a freedom from them must indicate a deficiency of moisture; that is, the air must be dryer. Now it appears from the *Ephemerides Meteorologicæ Palatinæ*, that the mean number of fair days, by observations made in twenty different cities, in different parts of Europe\*, amounted to only sixty-three, or sixty-four, and that the same year, at Cambridge, there were one hundred and seventy-three such days. To which I may add, that by my own observations at Salem, upon a mean of seven years, we had one hundred and thirty fair days annually.

The number of cloudy days in these same twenty cities was, in 1785, upon a mean, one hundred and thirteen, or one hundred and fourteen; but at Cambridge there were only

\* Viz. those mentioned in the annexed table.

sixty-nine; and at Salem, upon a mean of seven years, about ninety-five days annually.

The number of days, which were partly cloudy, in those same cities was one hundred and seventy-four, or one hundred and seventy-five; at Cambridge but one hundred and twenty-three; and at Salem annually, for seven years, one hundred and twenty upon a mean.

The number of rainy days in those cities was upon a mean 122; at Cambridge only 88; and at Salem 95 annually, for seven years.

The number of foggy days in those cities was sixty seven; at Cambridge sixteen; and at Salem, for seven years, twenty-one days annually.

As to hygrometrical observations, we unluckily have none to compare with the European ones, excepting only those made by the illustrious Dr. Franklin, and communicated in a letter to Mr. Nairne, on hygrometers, published in the 2d vol. of the Transactions of the American Philosophical Society at Philadelphia; by which it appears, that the air of Philadelphia is dryer, not only than that of Great Britain, but also than that of Passy, in France. This evidence seems to be direct.

Our evaporation then is greater than the European; our quantity of rain much greater; we have more clear fair days; we have fewer cloudy days, fewer foggy days, and fewer rainy days\*.

Thus, by every method of comparing the two atmospheres, the American appears to exceed the European in point of dryness. And although, perhaps, no one of all these facts, brought to prove our atmosphere dryer than that of Europe, does, when taken singly, determine any thing very satisfactorily, yet, when they are all fairly and candidly laid together, the proof arising from their joint evidence amounts to a very high degree of probability.

It may now perhaps be thought incumbent upon me to show how a greater purity and dry-

\* In the summer season, as there are more phlogisticating processes going on in Europe, to render the air foul, than in America, such as combustion, respiration, putrefaction, &c. so in the latter it is probable, that, at this season, the vast number of trees, in addition to the vegetables, which grow nearer to the earth's surface, in as great plenty as in Europe, must furnish a larger proportion of this purer air; so that in the hot as well as the cold seasons of the year, America must have the advantage of Europe in this particular.

ness of the atmosphere should occasion greater degrees of cold or heat ; or that I should point out the process of nature in generating heat or cold from dryness and dephlogistication \* : and many probable reasons drawn from chemistry, and many very plausible conjectures, might be adduced, to prove and illustrate this point. But as it seems generally supposed, that all the theories of heat, hitherto proposed, are rather imperfect ; or however that may be, as I must freely confess myself too little acquainted with its nature, to enter upon such a discussion, I would rather refer to observation and experiment.

Now it is, I believe, matter of constant and universal experience, at least in this country, and I suppose every where in cold countries, that the most intense cold always happens in the purest, dryest, and most dephlogisticated state of the atmosphere ; or, that we never

\* I have used the terms *dry*, *pure*, and *dephlogisticated*, as synonymous, or at least have considered them as qualities accompanying each other in the same state of the atmosphere. But that they are always necessarily and physically connected I do not pretend to assert ; that they commonly do accompany each other, I believe to be certain.



have our intensest frost, but when the air is in this state. That the air is very dry at such times, appears from the shrinking of wood, and all vegetable and animal substances, &c. That it is in a dephlogisticated state, appears from the rapid consumption of fuel, and the great tendency to scorch observable at such times in our ordinary fires; from the increased brightness and magnitude of the flame of candles and lamps; and from many other circumstances which might be mentioned. The weather, indeed, is frequently raw cold, as we vulgarly phrase it, and excessively uncomfortable, when the atmosphere is in a very humid state. The most disagreeably cold weather which we have in winter, happens when the air is in this damp state; but the thermometer at such times is never at or near its lowest stations; perhaps never nearer than 15 degrees, or upwards.

Farther, although the weather is frequently, during summer, most disagreeably hot and irksome to our feelings, when the air is very damp and phlogisticated, as appears by effects directly opposite to those just now enumerated, as the consequences of dephlogistication; yet so far as my observation reaches, the thermometer is never



never at its highest, at such times, but commonly 6 or 8 degrees below it\*.

Whence I think it may fairly be inferred, (whether we are able to account for it philosophically or not) that dryness or dephlogistication are, in fact and nature, necessary to the production of our intensest cold; and probably of our intensest heat. And if so, is it not natural to suppose, that when the atmosphere of any country is usually, both in summer and

\* It may, perhaps, have the appearance of paradox, to ascribe two such opposite effects, as heat and cold, to the same cause; but this appearance will, in a good measure, vanish, if it should be found, as I suppose it may, that dephlogistication produces cold, by its *chemical* effect upon the air: but that it produces heat *only mechanically*, by inducing a more perfectly pellucid state of the atmosphere, whereby fewer of the sun's rays are intercepted; and (as dephlogisticated air is specifically heavier by much than common atmospheric air) by occasioning a greater weight and density of the air near the earth's surface, whereby the sun's influence in producing heat is greatly increased. These considerations may serve to show, why cold is so much more increased by a dephlogisticated state of the atmosphere than heat. And it is observable, that the difference between two thermometers, one of which is exposed to the sun's direct rays, and the other in the shade, is always, *cæteris paribus*, much greater in a dephlogisticated, than in a phlogisticated state of the atmosphere.

winter,

winter, much dryer, and more dephlogisticated, that, *cæteris paribus*, it should be hotter in summer, and colder in winter there, than in that other?

But, allowing all that has hitherto been advanced upon this subject, I would not hastily conclude, that the superior dryness and dephlogistication of our atmosphere is alone sufficient to account for the whole of our superior heat and cold. There are probably other causes, which conspire with it to produce the same effect. I shall mention one, which I think of considerable moment.

All coasts which border upon a large ocean, in cold climates, must, during the season of winter, be warmed by winds which blow from the ocean upon them; plainly for this reason, that the waters of the sea in those latitudes never become so cold, by many degrees, as the surface of the earth: so likewise, in those same regions, the water of the sea never becomes, during the summer, so warm as the earth's surface; and therefore, at this season, winds blowing from the sea, upon the land, cool the air.

Now it appears by the *Ephemerides Meteorologicæ Palatinæ*, that the winds which are  
most

most prevalent in Europe, blow from the West, or at least from that semicircle of the horizon; more especially during the summer and winter months. Westerly winds, then, must cause the air of Europe to be warmer in winter, and colder in summer, than those that blow from the opposite quarter; because that continent lies eastward of the great Atlantic ocean. The directly opposite effect takes place in North America from the same cause; that is to say, the winds which prevail most with us, particularly in the hot and cold seasons, are likewise from the western quarter; for in the vernal and autumnal seasons they are commonly more variable, and blow more frequently from the eastward, than in summer or winter\*. We therefore feel less of the warming effects of the sea air in winter, as well as less of its cooling ones in summer; because our coasts lie westward of the ocean. Thus the winds which prevail most in Europe tend to mitigate both the heat and

\* Upon examining a number of American meteorological registers for a course of several years, I do not find more than one month, in sixteen or eighteen, in which easterly winds predominate; but I find seven or eight, in a year, upon an average, in which they blow almost constantly from the westward.

the cold, to which its geographical situation exposes it; as, on the contrary, the same winds increase both the cold of winter and heat of summer on the American coasts\*.

But upon the supposition that westerly winds are most prevalent in the middle latitudes all round the globe, which seems rather a probable conjecture, if we consider the facts just mentioned; and farther, that the course of the trade winds in the torrid zone is continually from the eastward, it ought to follow, that the eastern coast of Asia, as well as the eastern coast of America, should be colder than the western coast of Europe, or than the western coast of America, under the same parallel. And that such a difference does really obtain, seems to appear from the account given by the writer of Captain Cook's last voyage, who informs us that vegetation was in great forwardness, in the month of April, at Nootka, or King George's

\* Stockholm, in Sweden, lies in lat.  $59^{\circ} 20' N.$  and Tobolski, in Siberia, in  $58^{\circ} 12'$ ; yet it is found by observation, that the usual cold in the latter very much exceeds that of the former. Now, Tobolski is  $50^{\circ}$  of long. more easterly than Stockholm; of course so much farther from the Atlantic ocean. Doth not this observation confirm the truth of our hypothesis?



Sound, on the western side of N. America, in the latitude  $49^{\circ} 36'$  N. in the year 1778: whereas the next year, at the bay of Awatska, in Kamtschatka, on the eastern coast of Asia, in lat.  $53^{\circ} 38'$ , the snows were not gone, nor was there any appearance of vegetation, till the middle of May; which, if to be relied on, as the common course of things, is a strong confirmation of the doctrine just proposed\*.

But

\* It is a common observation, among those of our navigators who frequently traverse the Atlantic, in or near our latitude, that westerly winds are of all others the most usual; which has occasioned the sailors to call the passage from the eastward *uphill*. And it is observed, in Mr. Walter's Account of Anson's Voyage, that in the Pacific ocean, in the latitude of  $30^{\circ}$  or  $32^{\circ}$  North, the winds almost constantly blow from the westward, though in but moderate gales; but that in more northerly latitudes, as  $40^{\circ}$  or  $50^{\circ}$ , there are steady westerly winds; the writer therefore supposes, that the Acapulco ship might perform her voyage in much less time, if she stood farther to the northward, *where westerly winds constantly prevail*, than she does while she pursues her old track. These observations are additional proofs of the hypothesis advanced in this paragraph. Whether westerly winds prevail in the southern temperate zone, I know not; but if they do, the western coast of South America is probably warmer than the eastern, in latitudes similar to ours. If the course of the winds in our latitudes be generally



But it is more than time to close this paper, already much too long, which I shall do, after observing, that although I know not whether either or both the causes herein suggested may be judged adequate to the effects which I have ascribed to them, yet I think we must admit the operation of some partial or local cause (such as greater dephlogistication) to account for the greater degree of cold in Europe formerly, than at present---as well as of some general cause (such as the general course of winds from the westward in the temperate zone) to account for the greater degree of cold on the eastern confines of Asia, than on the western of America.

Be this, however, as it may, I flatter myself, that what is here offered may excite some per-

nerally from the west, will not this circumstance alone occasion the atmosphere of Europe to be more humid than the American, as the air from the sea must be more charged with the watery vapours, than the land air?

Varenius, in his Geography, page 609, 4th edition, Lond. informs us, that in "the north part of China, though  
" in a latitude not more northern than Italy, the cold feels  
" very sharp, and the great rivers and lakes are frozen."—  
and page 611, that "in Japan, which extends from 31° to  
" 39° N. they have a cold, snowy, wet winter."

sons of taste and leisure for such inquiries, to attend to the subjects here treated, and to examine with freedom the theory here advanced; that so, if it shall be found agreeable to reason and experience, it may be illustrated and confirmed; or, if otherwise, that it be confuted and exploded.

*Salem, September, 1788.*

### POSTSCRIPT.

Since the Academy did me the honour to read the above paper, I have had the pleasure of reading in the Philosophical Transactions, Vol. lxxvii. Article xv. an account of some very curious experiments made by our countryman, Sir Benjamin Thompson, at Manheim, in Germany; by which it appears, that eider-down, cotton wool, raw silk, &c. yield as much and as pure dephlogisticated air as the leaves of any kind of vegetables by the same process; that is, by exposing them, when immersed in water, to the action of the sun's rays: and therefore, that most probably this pure air is derived from the water in which they are thus immersed, and not from the substances, whether vegetable, animal, or mineral, which are  
thus

thus heated. Whence it seems to follow, that it is far from certain that any such pure air, or indeed any air at all, is derived from the leaves of plants exposed to the sun, as was suggested and seemed to be proved by the experiments of Drs. Priestley, Ingenhousz, and others. If this be really the truth of the case, and air is not produced from the leaves of vegetables, as in the paper just now mentioned I have supposed it to have been, then all the subsequent reasoning upon this hypothesis is void of foundation, and must fall to the ground. But it ought to be noted, that the facts and observations contained in the *Estimate*, which show our atmosphere to be really *dryer* than the European, are not at all affected by the failure of this hypothesis, but remain in their full force, though I may have mistaken the cause when I attributed it to the purity of the air derived from the leaves of vegetables.

Farther, since writing the paper before mentioned, I have accidentally been informed of a fact, which confirms the idea that our evergreens are, if not the cause of dephlogisticating the air, yet somehow the cause of an increase of cold. The fact I mean is, *that frosts are commonly observed to appear much earlier every autumn,*

*tumn, as well as later in the spring, in the neighbourhood of pine and other evergreen woods, than in other places, or than in the neighbourhood of other woods which drop their leaves in the winter. And this I find confirmed by every one I have since inquired of, whose business or situation leads them to attend to the matter; and I am told, it is a common observation, though I confess I never heard of it, before I presented the paper to the Academy.*

If this observation be well founded, then (whatever may be the fate of Dr. Priestley's and Dr. Ingenhoufz's experiments) our pine woods are a source of cold which Europe is now in a great measure deprived of; as there is no doubt but that trees, of the evergreen as well as of every other kind, are now few, and thinly scattered over that continent, compared with what they must have been in past ages, or than they are in America at present.

*Salem, November, 1790.*

*A TABLE of the greatest HEAT and COLD, and of the Mean of the greatest Heat and Cold, collected from Observations made for a course of Years in twenty different Cities in Europe; as exhibited in the Ephemerides Meteorologicæ Palatinæ: and at Salem, in North America, for seven Years: shewing the Excess both of Heat and Cold in America, beyond that of Europe in the same Latitudes; by Fahrenheit's Thermometer.*

| <i>Names of Places</i> | <i>Lat. north</i> | <i>L. of years.</i> | <i>Greatest Heat by thermom.</i> | <i>Mean of the greatest heats.</i> | <i>Greatest Cold by thermom.</i> | <i>Mean of the greatest Colds.</i> |
|------------------------|-------------------|---------------------|----------------------------------|------------------------------------|----------------------------------|------------------------------------|
| Stockholm              | 59:20             | 3                   | +87,80                           | +83,98                             | -16,37                           | -10,19                             |
| Copenhagen             | 55:40             | 4                   | +85,10                           | +81,77                             | + 0,98                           | + 2,98                             |
| Berlin                 | 52:32             | 4                   | +95, 0                           | +89,37                             | - 3,55                           | - 0,62                             |
| Sagan                  | 51:42             | 5                   | +96,35                           | +90,86                             | -21,32                           | -12,23                             |
| Erfurt                 | 51:04             | 5                   | +94,10                           | +89, 6                             | -14, 1                           | - 3, 1                             |
| Mons                   | 50:25             | 5                   | +95, 0                           | +89,15                             | - 7,37                           | + 1,18                             |
| Prague                 | 50:04             | 4                   | +96,35                           | +92,07                             | -17, 5                           | -12,77                             |
| Wurtzburg              | 49:46             | 5                   | +102,4                           | +93,87                             | -18, 4                           | - 4, 0                             |
| Manheim                | 49:27             | 5                   | +93,15                           | +89, 6                             | - 8,95                           | + 1, 2                             |
| Ratisbon               | 48:56             | 4                   | +96,57                           | +79, 7                             | -13,45                           | - 2,42                             |
| Buda                   | 47:40             | 4                   | +92,75                           | +90, 7                             | - 2, 2                           | + 4,26                             |
| Peiffenberg            | 47:47             | 5                   | +84,35                           | +76, 1                             | - 8,95                           | + 0,28                             |
| M. St. Andex           |                   | 5                   | +89,60                           | +89, 8                             | - 7,60                           | + 1,13                             |
| Tegernsee              | 47:37             | 5                   | +87,12                           | +83, 5                             | -12,32                           | + 0,73                             |
| St. Gotthard           | 46:31             | 4                   | +66,87                           | +62, 3                             | -10,75                           | - 4,67                             |
| Geneva                 | 46:12             | 3                   | +92,75                           | +88, 9                             | + 1,85                           | +10, 2                             |
| Rochelle               | 46:09             | 4                   | +93, 9                           | +90, 5                             | +10,63                           | +16,93                             |
| Padua                  | 45:22             | 5                   | +97,25                           | +91, 4                             | + 7, 7                           | +16,93                             |
| Marseilles             | 43:17             | 3                   | +90, 5                           | +89, 6                             | +24,13                           | +27, 5                             |
| Rome                   | 41:53             | 4                   | +86,67                           | +85,43                             | +31, 1                           | +33,46                             |
| Mean                   |                   |                     |                                  | +86,41                             |                                  | + 3,31                             |
| Salem, N. A.           | 42:31             | 7                   | +106                             | +97,02                             | -11, 0                           | - 2,42                             |



XX. *An Account of an uncommon Case of Emphysema; and of an external Abscess, the Contents of which were discharged by coughing.* By Edward Augustus Holyoke, M.D. F.A.A. — From the *Memoirs of the American Academy of Arts and Sciences*, Vol. II. Part I. 4to. Boston, 1793.

ON Tuesday, July 15, 1783, I was desired by the gentleman who attended him, to visit a boy of about twelve months old, who he told me had been most severely handled by a peripneumony for ten days; but that a very uncommon tumour lately appearing upon the child, made him desirous I should see him.

Upon viewing the patient, I found a large, soft, elastic, flatulent tumour, evidently crackling under the fingers when pressed upon, as tumours arising from air in the cellular membrane usually do. This tumour shewed itself all at once on the preceding evening, about seven or eight o'clock, as the child lay in its mother's lap, during a violent fit of coughing,

on one side of the neck, near the right mastoid muscle; and by ten o'clock the next morning, when I first saw it, it had spread across the neck, and up by the left ear, under the scalp, so as to cover the whole extent of the crotaphite muscle on that side; on the left side it extended no farther up than the ear; downwards, it spread on the breast below the paps on both sides, and to each axilla, and was evidently increasing, especially upon coughing.

The pulse was frequent, the flesh hot, the respiration deep, frequent, and laborious to a great degree; the cough frequent and violent; and the mouth lined with aphthæ; but none of these symptoms were increased (as I was informed) since the Emphysema had taken place: the child appeared in the utmost danger.

About one o'clock the same day, I saw the child again; the symptoms continued much the same; but the flatus had spread on the left side of the head up to the vertex; and farther down upon the breast; and farther round than the axillæ.

The next morning, Wednesday, the 16th, the Emphysema occupied a larger space upon the head, though chiefly on the left side still; had extended over both scapulæ; and had got

farther down on the breast, and indeed covered the whole thorax on the fore part, and on the left side passed over the oblique abdominal muscles down to the groin. The child had now a more cadaverous look; the hands were purplish, and the pulse plainly lower and more sunk; the difficulty of breathing still kept up; and we expected he would soon expire.

Thursday, 17th, A.M. Matters in much the same situation as yesterday, only the tumour now extended over both sides of the abdomen, but did not pass over the recti muscles; covered almost all the back, and on the head had passed over the vertex, and now covered the whole right side.

Friday, 18th, the child still alive; the dyspnoea, cough, &c. still continued. He had taken no medicine for twenty-four hours past, nor had swallowed any thing but a little drink. The tumour now covered the whole trunk, except a small area round the navel, and a narrow stripe both above and below it, upon the linea alba, which were free from all swelling; the whole neck was puffed up, and the head under the hairy scalp every where, except the back part, where the air seems to have been prevented from insinuating itself, by the pressure  
: of

of the head, as it lay upon the pillow; the face was every where free, as also both the upper and lower limbs. This evening the child died; but, to our great mortification, no persuasions could prevail upon the mother to permit the body to be inspected. The appearance of the tumour upon the dead body was much the same the next morning as it had been before death.

To account for those uncommon appearances, I think we must suppose a communication somewhere between the cavity of the lungs and the cellular membrane; and as the first appearance of Emphysema took place in the neck, upon a violent fit of coughing, it seems highly probable that this communication was formed by a rupture of the membranes of the *aspera arteria*, somewhere between its cartilages\*; and thus gave passage to the air from the lungs into the adjacent cellular membrane, at every expiration; and as the cough was very violent, the air would, at every such effort, more especially, be forcibly impelled through this opening, and

\* Possibly a small abscess might be formed between these membranes, and so, by weakening them, occasion their bursting, upon a violent exertion in coughing.

thus extend itself wherever this membrane extended; at first indeed more rapidly, but still continue to extend, till the resistance which the air met with, in passing out at the opening, was equal to the force by which it was expelled from the trachea in expiration or in coughing.

This solution of these appearances cannot be ascertained, as we were not allowed to open the body, and whether it will be thought admissible, I cannot determine; but as another case which fell in my way not a great while ago, may throw some light upon this, I will take the liberty to relate it.

A man, about fifty-three or fifty-four years old, of a thin habit of body, labouring under a very bad cough, attended with a hectic fever, profuse sweats, &c. had a large tumour formed upon the upper part of the thorax on the left side, extending from the shoulder, all along the lower edge of the clavicle, to the sternum, about the breadth of a man's hand. This tumour had all the appearance of a large abscess; it was accordingly treated as such, and suppuration seemed to be coming on as usual; but on removing the dressings one day, I found the tumour (though the skin remained whole) less prominent to the eye, flabby to the touch,



and the pain and inflammation abated. I was now at a loss what to make of the case, as the abscess seemed too far advanced to expect discussion. While I was thinking of the matter, the patient asked me, “ What could occasion “ that blubbering noise (as he expressed himself) in the fore? Upon which, applying my ear near the part where he perceived the noise, I plainly heard a whizzing, and, as he termed it, a blubbering noise at every breath, exactly resembling such as arises from the rushing of air through a small orifice. This orifice appeared to be just under the left clavicle, but nearer to the shoulder than the sternum. Upon viewing the part attentively, a small dilatation and contraction were perceptible upon expiration and inspiration; and the part was evidently puffy and flatulent to the touch. At this time the cough was urgent, and the expectoration very copious.

From this time the tumour, inflammation, and hardness subsided; the noise in breathing gradually lessened, till it ceased; and by the assistance of pectoral medicines, the bark, &c. the hectic and cough after a while left him, and with them the sweats, &c.; his appetite returned,

returned, and he recovered his strength, though slowly, and is at this time in tolerable health.

In this case I think it certain that the inflammation penetrated to the lungs, which, no doubt, adhered to the pleura in this part; and the abscess bursting inwardly, the matter was discharged through the trachea by the assistance of the cough, which was at this time very constant; but the cavity of the lungs having now a communication with the cavity of the abscess, some of the air from the lungs would pass at every expiration into this cavity; but would not diffuse itself in the cellular membrane, and produce general emphysema in this case, as in the case first mentioned, probably because the inflammation of the cellular membrane, which surrounds all abscesses, and limits their extent, must have formed a barrier impenetrable by the air, as it rushed out of the lungs into this cavity; and of course the whole of what was thrown into the cavity of the abscess at each expiration, would be drawn back again into the lungs at the next inspiration, and thus the surrounding parts might escape tumefaction; and this passing and repassing of the air will fully account for the noise which the patient complained of.

XXI. *Account of a Locked Jaw.* By Aaron Dexter, M. D. F. A. A.—From the *Memoirs of the American Academy of Arts and Sciences.* Vol. II. Part I. 4to. Boston, 1793.

I BEG leave to present to the Academy a particular history of the unfortunate case of my friend, Dr. Edward Wyer. It rarely happens that the particular circumstances attending the disease, with the full effect of applications, and a constant variation of practice, as symptoms appeared, can be attended to, as was the case in this instance; owing to his having no other nurses than such physicians, with his own assistance, as were able to change the mode according to appearances.

I presume it may give some information to medical gentlemen, who have not had an opportunity of being witnesses to such distressing scenes.

I am, with the greatest respect,  
Your most obedient servant,

A. DEXTER.

The Hon. JAMES BOWDOIN, Esq.  
President of the A. A. S.

*An Accurate History of a Locked Jaw, from a wounded Membrane, that terminated fatally.*

**E**DWARD WYER, a gentleman of the medical profession, trod upon a shingle nail, which passed through his shoe, into the ball of his left foot, directly over the tendon leading to the second toe. A few drops of blood followed. He applied only a little petroleum to the part.

The next day, (August 26, 1789) a slight lameness ensued, with nausea at the stomach. The third day, it was apparently well; and he attended his usual business without any regard to his foot.

Sunday, September 7th, whilst at dinner, he observed some difficulty in swallowing. Monday the 8th, he perceived a considerable stiffness in the muscles of his neck. On Tuesday the 9th, it increased, but not so much as to prevent his riding ten miles in the afternoon: and he concluded, he was attacked with the mumps. He applied, in the evening, volatile liniment to his neck and jaws. During the night, he was restless; and after sleeping, repeatedly found, that he had bit his tongue.

Wednesday

Wednesday morning the 10th, he perceived a stiffness in his back, as though he had been sleeping on a board ; and could scarcely open his mouth. Upon his attempting to speak, or swallow, spasms seized his throat. At this time, he was fully convinced that he had a locked jaw: and the affair of the nail was recollected, with all its circumstances.

During the forenoon, his disorder increased rapidly. At twelve o'clock, a medical friend called on him by accident ; he made an effort to speak, but could not for some minutes. As soon as he was able, he related his case, and asked advice. They conversed on the several different remedies that have been recommended. Opium he conceived of no avail: he said, that he had made patients under his care, take it in large quantities. The warm bath was mentioned; but he objected to it. He agreed, that a cold bath was the only thing that could save him. Mercurial frictions were mentioned; but he would not consent; and answered, that he had often tried them without any sort of advantage. He then recited the cases in the 6th vol. of the London Medical Observations, proving the good effects of cold water; and, at the same time, produced his own minutes of a case of a locked jaw, cured  
appa-



apparently by cold bathing, which took place about twelve months before in a neighbouring town.

On examining his foot, nothing was perceived but a very slight speck where the nail entered. There was no soreness, tenderness, or pain in that part, more than in any other. It was agreed to apply the cold bath immediately. He placed himself on a low stool, naked; and two buckets of cold water were thrown on his head: after which, he placed himself between two blankets on his bed. An agreeable warmth soon took place; and he expressed relief, from the application, particularly at his stomach; and could swallow better.

The first application was about one o'clock in the afternoon; and it was repeated exactly in the same manner, four times.

At six o'clock, he was evidently relieved. After the fourth bath, he rose from the stool, with great satisfaction; could speak with ease; and drink without difficulty. During the five hours, he took liberally of broth and gruel. The fifth bath had a very different effect: it produced a tremor, and great anxiety. Spasms attacked him more violently than ever, particularly on the back of his neck; which was embrocated  
with

with oil of cloves, diluted with spirits of wine. The spasms were very violent also in the muscles of his jaws. To prevent his mouth closing completely, which he apprehended, he had introduced a stick between his teeth; and this was of great importance to him, during his life. At this time his foot was examined, and the speck taken out; which did not show any trace of the nail under the scarf skin. A blister, as strong as could be made, was applied to the part: but he was utterly averse to having the tendon examined; conceiving it too late to make any application to the part originally affected, as the disease had become general.

He wished to have the system supported with wine, bark, and nourishment.

The bath of cold water was tried again, at eight o'clock; but it evidently increased his disorder; and from that time, he would never consent to its application.

During the night, several enemata were administered, of a strong decoction of red bark and snake root. At his usual bed time, an anodyne of two grains of opium was given him: he passed a very restless night. He soon found, that cold drinks produced less spasm than warm;  
which

which led him to take every thing that he was able to swallow, cold.

Thursday the 11th, it was proposed to him, to pass a seton, covered with cantharides, under the skin of the affected part. To this he consented, with a design to inflame the part : but it produced no good effects ; it seemed rather to increase his irritability. In the course of the day, three enemata were administered of a decoction of bark, as before.

He had a great aversion to bark in any form, received into his stomach ; as it generally, in health, produced nausea. The object of this application was to give tone to the stomach ; from a presumption that debility was the immediate effect of the disease.

The gentlemen of the medical profession, who were present, suggested to him the use of the warm bath ; as every other application had proved of little effect ; to this he consented. About four o'clock, in the afternoon, he was placed in a bathing machine, with water heated to 90° of Fahrenheit's thermometer, in which he remained seventeen minutes. It produced a good effect, in relaxing the muscles in general, particularly of his body and arms. Growing faint, he was taken out ; covered with flannel ; and put on his bed. A most profuse diaphoresis ensued ;

fued ; and he felt fo much relieved, that he faid, he then had a fecret hope that he fhould recover: but, within an hour, on attempting to drink fome lemonade, his fpafms returned as violently as before, and were more general ; but feemed to remit at fhorter periods.

At eight o'clock, he was anxious to try the warm bath again ; and was placed in it as foon as poffible, but without any good effect. He could bear it but a few minutes, before he became faint ; and fpafms attacked him in this fituation. He paffed a better night than he expected ; and obtained fome fleep, by keeping his head accurately balanced.

Friday the 12th. This morning he feemed better ; his fpafms were not fo violent ; and he was much encouraged. A laxative enema was adminiftered ; as nothing had paffed his bowels fince Wednefday morning. In the afternoon, fpafms returned more violently than ever, and were more general. The warm bath was again ufed, but without procuring any relief : and he paffed a very diftreffing night.

Saturday the 13th, a cathartic of calomel was propofed, which met his approbation. It is to be obferved that he now preferved his reafon entirely,

tirely ; and was unfortunately able to judge for himself of his situation, and the full effect of every application. He had, from the first moment, considered his case as out of the reach of medical assistance.

This morning, electricity was proposed, which he approved ; and such sparks were drawn as he was able to bear, without producing spasms, from the parts most affected. The electric fluid was passed through him, in various directions, for about one hour. He thought himself calmer in consequence of the application ; and passed the day without violent spasms. Electricity was repeated in the evening, but without any apparent effect. His sensibility had much increased since Thursday night. Constant attention was necessary, from the physician, to keep the muscles exactly balanced.

In the evening, it was agreed to make use of mercurial frictions ; as there had been some similar cases related, in which this application had succeeded. It was used through the night very freely. A laxative enema was also administered, but without effect. His thirst was very great. From 9 o'clock in the evening, to 6 o'clock the next morning, he drank five pints of cold water, and as much lemonade,



Sunday 14th. A discharge from his bowels was produced; but was not considered as sufficient: and ten grains of calomel were given him, with one hundred drops of laudanum. He passed a tolerable day without any violent spasms; took but little food, as his stomach nauseated it; but drank cold water and lemonade in large quantities. At 4 o'clock in the afternoon, an enema was administered of broth and half an ounce of laudanum. It was agreed to omit the mercurial frictions; and keep him as quiet as possible; and to give him as much food as could be retained on his stomach, or by his bowels. The laudanum soon had an effect. At 5 o'clock, he lay quietly sleeping under its influence. Appearances were favourable at this time, in the opinion of all the medical gentlemen present. He continued quiet, and slept easy till ten o'clock; when a laborious respiration took place. An attempt to awake him was made without effect; and the difficulty seemed to increase very fast. He was then raised up in his bed; and carried to a chair, without any signs of life, except an interrupted catching for breath, and a very feeble pulse. The most stimulating volatiles were applied to his mouth, nose, temples, &c. without any effect. At 11 o'clock, his respiration was scarcely

scarcely perceptible; and his pulse intermitted. He was laid on his bed as a dead man. In a few minutes, his pulse seemed more connected. He was raised up on the side of the bed; all the windows were opened; and an enema was administered, of a solution of cathartic salt in strong peppermint water, which, in a few minutes, operated very largely; and part of the laudanum was evacuated. His respiration gradually recovered; and his pulse rose full. These circumstances induced his medical attendants to repeat the enema as before; at half past 12 o'clock, he was again placed in his bed, and breathed tolerably easy; and had a copious involuntary discharge. The external stimulating applications were continued; and his spasms returned in a slight degree, just sufficient to lock his jaws during their continuance. Every muscle had been perfectly relaxed since 10 o'clock. His respiration grew gradually better; and at 3 o'clock, he was able to speak; and found to his great surprise, the muscles of his jaws relaxed, and as free from spasm as ever. His thirst was violent: he drank, from 3 o'clock to half past 5, one quart of cold water, two quarts of lemonade, and a bottle of spruce beer. After this he slept quietly half an hour. There seemed to be a

ſingular alteration, and he was very much elated; and fully believed, that a complete criſis had taken place. He continued free from any ſpaſm, particularly in his jaws and neck, till ſeven o'clock in the morning.

Monday 15th. At 8 o'clock his left leg and thigh were extremely affected with ſpaſms. The violence and pain of them were ſo great, that during three hours, he was, for the firſt time, deprived of his reaſon. At the intervals he begged for an enema of laudanum, as the only thing that could ſave him from the ſevereſt torture. Electricity was firſt tried on the part, but without any effect. Afterwards, an enema was given with one hundred drops of laudanum. The ſpaſms of the abdominal muſcles forced it from him immediately. His ſenſibility was ſo extremely increaſed, that opening a door, walking in his room, or ſpeaking louder than a whiſper, would produce ſpaſms too diſtreſſing for language to expreſs. Soon after the clyſter came from him, he had ſeveral free diſcharges; and a diarrhœa took place, which laſted through the day. In the evening, a julap of oil of cinnamon, and thirty drops of laudanum, was given, which checked the diſcharges; but he paſſed a very  
reſtleſs

restless night. Towards morning his spasms relaxed, and he slept a little.

Tuesday 16th. This morning he seemed tolerably easy. At 12 o'clock, a spasm seized his diaphragm and lungs. Extreme difficulty of respiration came on; and he appeared to sink under his complaints; took leave of his family; and made several arrangements respecting his property, and his funeral, with great composure: satisfied that, from the parts attacked, it was impossible for him to live but a few hours. Vescicating tincture of cantharides was applied on his breast; and a tea spoonful of Hoffman's anodyne mineral water was given him, without any effect. When life seemed just quitting him, a large discharge of flatus from the intestines, followed by a fetid discharge of excrement, gave immediate relief. An enema of a solution of cathartic salt was administered, which gave him two discharges. He seemed totally disappointed in being thus relieved; and considered it as a singular medical change.

At seven o'clock in the evening, he took a large spoonful of Huxham's tincture of bark, with two spoonfuls of wine; which proved very grateful. His pulse was very feeble, but his spasms seemed to have left him altogether. It

was agreed, that he should repeat the last-mentioned medicine every hour. He asked for cold cider, which he found very grateful to his taste. At 8 o'clock, he repeated the tincture of bark and wine; and asked to be turned in his bed, which was immediately done. He perceived a spasm, and called for a pillow to raise his head a little; which being placed agreeably to his wish, he stretched himself out during the spasm; and his respiration and pulse ceased instantly, without the least emotion. The medical gentlemen, who constantly attended him, supposed that a spasm seized his heart, which deprived him of life.

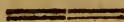
Wednesday morning. As it had been invariably his request, that, after his death, his foot might be examined, his family consented. The skin and cellular membrane were removed; and the nail could be traced to the sheath of the tendon, which was found to have been perforated; it did not enter the tendon; but passed by the side of it to the periosteum of the bone, which it had not affected.

Under the tendon was a small cavity, about the size of a pea, discoloured throughout, with evident marks of previous inflammation.

The phalanx of the toe was taken off; but no



further discovery was made. From Wednesday, the 10th of September, to his death, he never was without some medical friend in his chamber. And from Friday morning, he had two, and frequently three or four with him. His situation was such, that without some person well acquainted with the profession, his distress must have been exceedingly increased.



XXII. *An Account of the Effects of Negative Electricity, in Cases of Burns.* By Mr. John Vinall.—From the *Memoirs of the American Academy of Arts and Sciences*, Vol. II. Part I. 4to. Boston, 1793.

**I**N making use of my large electrical machine, which is constructed with both a positive, and a negative conductor, the air being humid, and consequently unfavourable for electrical experiments, I made use of a small iron pan with some coals under the machine, in order to qua-

lify the surrounding atmosphere, so as to answer my purpose. By accident I burned my thumb with the pan so much, as to cause me great pain. Knowing that in some instances, I had been relieved of slight burns, by holding the part affected to a common fire, I held my thumb at a small distance from the *negative* conductor; put the machine in motion; and to my surprise found, that in a few seconds of time, the effects of the burn were destroyed; that my thumb was perfectly at ease; and that no blister arose, as would, I think, have been the case if I had not made use of electricity. I met with a similar instance not long after: I made use of the same remedy, and received the same benefit.

A few weeks after this discovery, one of my daughters scalded her arm from her wrist to her elbow, with the steam from a tea kettle, which produced a great inflammation upon the part, attended with much pain; and it is highly probable a blister would have succeeded. I desired her to hold her arm to the *negative* conductor; and in a few minutes, the pain ceased, the redness subsided, and her arm was perfectly cured.

I never read, or heard of an experiment of this kind being made in electricity: therefore, I esteemed it my duty to lay it before the Academy

demy of Arts and Sciences, that gentlemen might be induced to make experiments from this hint, which may be of great service to mankind, and an improvement in medical electricity.

*Boston, May 23d,*  
1790.

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XXIII. *Description of a Case of Hydrocephalus. By M. Tenghil, Professor of Surgery at Quiers. Vide Memoires de l'Academie Royale des Sciences. Années 1790-91. 4to. Turin, 1793.*

THE subject of the singular instance of hydrocephalus here described was a male child, born at Peceto, a hamlet, near Quiers, in Piedmont, on the 21st of September, 1790, with a tumour or cyst hanging down from the back part of the head to the shoulders, but without any other apparent defect of structure. The father of the child brought

brought it to M. Tenghil three days after its birth, to consult him about this tumour, which was as large as the child's head. Its shape was spherical, and it was connected by a narrow basis, of about an inch in width, to the left side of the occiput. The skin was of its natural colour. Some hairs, which grew on the upper part of the tumour, covered about a third part of it. The veins on its surface were much distended, and there were here and there livid spots, which our author supposed to be the effects of pressure. No very distinct fluctuation, he observes, could be felt. On a suspicion that it might be a case of hydrocephalus, he examined, he tells us, the basis of the tumour, to see if there was any part of the occipital bone wanting; but he was unable to ascertain this point: and as the futures of the head did not appear to be preternaturally dilated, he candidly acknowledges he was unable to satisfy his mind completely with respect to the precise nature of the tumour. He was convinced, however, that nothing could be attempted for the relief of the child, and that it could not live long; and this opinion he gave to the father.

The child died on the 25th of October, and  
our

our author had then an opportunity of ascertaining the state of the parts by dissection.

The size of the tumour had increased a little from the time he first saw it. Its length was now rather more than three inches; and, at its largest part, it measured about seven inches and a half in circumference. It was now, however, no longer of a spherical shape, but was become in some measure flaccid. Its outer surface was excoriated, and covered with a foetid discharge. A fluctuation was now more distinctly to be felt. On opening the tumour, a fluid was discharged, of a yellowish colour, inclining to red, in a greater quantity than the bag itself seemed to be capable of containing. On the right side of the bag, close to the occiput, there was a fungus as large as a walnut; and on touching the upper part of the cavity, contiguous to this substance, there seemed to be a deficiency of bone, but the opening in it was too small to admit the author's finger. He therefore introduced a probe, and found that it passed into the cavity of the cranium. He now carefully traced this opening in the os occipitis, and found it to be not more than three or four tenths of an inch in width, and situated at the side, and a little to the left, of the tuberosity  
of



of that bone. It was through this opening that a greater quantity of fluid was discharged than the cyst itself was capable of containing. This fluid had pressed in part on the tentorium of the cerebellum, and had so macerated the left posterior lobe of the brain, that it dissolved on the least touch. The middle lobe of the brain, on the left side, was also softened by it.

The dura mater came out through the opening; but our author, from the macerated state of the parts, was unable to ascertain whether it lined the whole inner surface of the bag, or had been torn by the weight of the fluid. He was able, however, to trace the fungous substance, through the opening, to its termination a little above it on the surface of the brain, and it seemed to him to have been produced by the cellular texture of the pia mater. The other parts contained within the head were in their natural state.

M. Tenghil has contented himself with giving, without any comment, this concise description of the appearances in question, illustrated by two engraved figures, which our readers will find copied, on a reduced scale, in Plate II. One of these figures (fig. iv.) shows the external appearance of the bag; and the  
other

other (fig. v.) its internal surface. In this last figure, *a* refers to the opening in the os occipitis, and *b* to the fungous substance above described\*.

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XXIV. *Account of a Case in which a Stone, formed in one of the Kidneys, was extracted through an Abscess in the Back.* By Herman Schützer-crants, M. D. Vide *Kongl. Vetenskaps nya Handlingar*, Tom. XII. 8vo. Stockholm, 1791.

THE patient, whose case is here described, was a widow, aged fifty-six years, who, for several months, had been subject to violent pains in her back and loins, when our author was first called to her assistance, in consultation with her physician, Dr. Lindberg, who had prescribed for her different remedies, but with-

\* We find a case, in some respects similar to this, described and delineated by Job a Meek'ren, in his *Obs. Medico-Chirurg.* p. 53. 8vo. Amstelodami, 1682. See also Ruysch *Obs. Anatomico-Chirurgic. Centur.* (Obs. 52. fig. 45) 4to, Amstelod, 1737. EDITOR.

out being able to relieve her complaints. This was in August, 1777. She had then a quick full pulse, with alternate shiverings and heat, and other symptoms of fever; and was subject to frequent vomiting. She complained also of a swelling which was found to extend over the whole of the right hypochondrium. This swelling was soft and œdematous, without any discoloration of the integuments.

Venæsection, purgatives, opiates, and other remedies adapted to the symptoms, were had recourse to, but without procuring any permanent relief to the patient. At the end of three weeks, from the time our author first saw her, the swelling was considerably increased, and the pain and feverish symptoms were become more violent. Instead of being every where soft, as at first, there was a considerable hardness to be felt in the tumour, immediately under the short ribs, and when this part was pressed with the finger, the patient complained of great pain.

Different plasters and cataplasms were applied to this tumour, with a view to promote suppuration: and in the course of ten or twelve days more, a fluctuation could be distinctly felt. The integuments now became gradually thinner and

and more pointed, and the abscess being at length opened with a bistoury, more than a quart of matter was discharged from it.

Our author introduced a probe into the abscess, but was unable to ascertain the full extent of it. Suitable dressings and bandages were applied to the part, and for some days there was a good discharge from the abscess, and the pain and feverish symptoms subsided; but at the end of a fortnight the discharge became thin and even watery, wetting the compresses through, excoriating the integuments, and diffusing a smell of urine. Our author was now convinced that this discharge came from the kidney; but still, he observes, he had no suspicion of a stone, till, on introducing a probe again into the abscess, he felt it in contact with a hard, rough, and seemingly broad substance. The orifice of the abscess being now too small to admit his finger, he enlarged it with a bistoury sufficiently for that purpose; but still he was only just able to reach the stone with the end of his finger. By means of a pair of forceps, however, he at length succeeded in extracting the calculus delineated in Plate II. fig. vi.

After the extraction of this stone, the discharge of urine from the abscess gradually diminished;

minished; and at the end of a fortnight entirely ceased. The wound now began to yield a laudable pus; the patient's strength was supported by Peruvian bark, and the wound gradually healing, she at length found herself free from complaint.

From inquiry of the patient, our author learnt, that previously to this complaint she had been subject to the Gout; but had never observed any stone or gravel in her urine.

This case, he observes, proves, that the extraction of a stone from the kidneys, or what is called nephrotomy, is, as different writers have already remarked, practicable; but it can, he thinks, be advisable only in instances like the present, when matter formed within the kidney is able to make its way to the surface of the body, and thus points out to the surgeon the tract he has to pursue.

Dr. Schützer crants refers his readers for some useful observations on this subject\*, to a

\* The reader will also find an instance, by the writer of this note, of urinary calculi making their way from the kidney through a fistulous abscess in the loins, inserted in the *Philosophical Transactions*, Vol. LXIV. Part I. for the year 1775.  
EDITOR.



paper by Mr. Hevin, entitled *Recherches historiques et critiques sur la Nephrotomie, ou Taille du Rein*, inserted in *Memoires de l'Academie de Chirurgie*, Tom. III. p. 238. 4to. Paris, 1751.

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XXV. *An Account of the poisonous Quality of the Juice of the Root of Jatropha Manihot, or bitter Cassada; and of the Use of Cayenne Pepper in counteracting the Effects of this and some other poisonous Substances; with Remarks on the Efficacy of the Spigelia Anthelmia in Worm Cases. Communicated in a Letter to Dr. Simmons, by James Clark, M.D. Physician in Dominica, and Fellow of the College of Physicians and Royal Society of Edinburgh.*

THE pernicious quality of the juice of the roots of *Jatropha Manihot*, or bitter Cassada, has been known ever since these islands were first cultivated by Europeans. It is not certain whether this plant is indigenous, or whether it has been brought from Surinam, Demerary, or

some other part of South America, where it is planted, prepared, and used as bread by the Indians, in the same manner as it is by the red Caraihs, or true Aborigines of these islands, at this time.

From the very sudden effects of the juice of the roots of this plant on negroes who had drank it intentionally, or on those who had eaten the roots by mistake; and also from the fatal and speedy effects of it on horses, mules, cattle, goats, sheep, and on all kinds of poultry, which I have been an eye witness of, I have been induced to consider it as the most powerful narcotic vegetable poison that we are acquainted with at present, not excepting even the cherry laurel water. The Indians of South America, and the Caraihs of these islands, who appear to be the same race of men, boil this juice with Cayenne pepper and salt, which they use as a sauce to their fish, and soak their Cassada bread in it before eating it; from which circumstance I was led to make a few experiments, in order to ascertain whether boiling destroyed the poisonous quality of this juice, or whether its effects as a narcotic were counteracted or destroyed by the pepper.

In July, 1794, I procured four large bull  
frogs,

frogs (*Rana latrans*,) and after grating several roots of Cassada, and pressing out the juice, I put half a pint of it apart, and boiled a pint of it slowly, till it was reduced one half, of which, after letting it cool, I gave four tea spoonfuls to two frogs, and the same quantity of the raw juice to two more; not more than one half remained in the stomachs of each. Three hours after, the two frogs that had swallowed the raw juice died. I repeated the dose of the boiled juice to the two others at this time, and one of them died in an hour after, but the other survived some hours; it was, however, motionless, and insensible to the prick of a pin at the time the other died. From this I found that the boiled juice poisoned as well as the raw, although not quite so soon. But suspecting that the juice had not been boiled sufficiently, I had some more boiled down to one fourth of the quantity, when it became a little thick, resembling a thin jelly of starch. I gave four tea spoonfuls of this juice to two frogs as before, and the same quantity of raw juice to two more. One of these that took the boiled juice died in three hours, and the other in the night, after having had the dose repeated; as did those which had taken the raw juice: it might be about five hours

from the time the juice was given to each of them, that they died. It would seem from these trials, that the boiled juice poisoned sooner than the raw ; but upon repeating these experiments frequently, it was found, in general, that the raw juice was more quickly fatal. The difference in this instance arose probably from the whole quantity of the juice not reaching the stomach, or from their having rejected a great part of it after it had been swallowed. This happened, more or less, to be the case in all the experiments upon them.

As these animals are known to be remarkably tenacious of life, I thought it might give some idea of the great power of this poison, to know how long they would live after having been deprived of their vital parts. With a view to this object, I cut out the heart of a frog, and stitched up the thorax immediately, and at the same time I cut off the head of another frog ; the heart continued to beat an hour, and both frogs lived near two hours ; so that they were killed nearly as soon by the Cassada water, as they were by destroying parts so essential to life.

I poisoned lizards also with this juice, but the effects of it upon animals with cold blood,

was

was not near so sudden as on those with warm blood, such as sheep, kids, turkies, fowls, &c.

I have known a strong negro die in little more than an hour after drinking perhaps half a pint or more of this juice; and a strong mule in much less time. Negroes who had eaten the roots roasted, lived three or four hours after. Finding, from these trials, that neither boiling nor roasting destroyed the poisonous quality of the Cassada root, I suspected that the capsicum, or Cayenne pepper, boiled in it by the Caraihs had this effect, and that it might be the real antidote for it. I therefore procured more frogs, to some of which I gave the raw juice and the boiled, as in the former experiments, and with the same effect; to others I gave the juice boiled with a sufficient quantity of capsicum to make it taste pretty hot. The frogs that took the juice in this way lived, and seemed to be enlivened by it every time it was repeated, which was very frequently, and they did not die afterwards.

The white people in South America use this sauce with their fish, as the Caraihs do in all the islands where they reside, without suffering any inconvenience or bad consequence from it. It appears, therefore, that the action of the capsi-



cum, as a powerful stimulant to the stomach, and to the system in general, counteracts the sedative or narcotic powers of this juice. Capicum has been known, long ago, to possess the power of counteracting or preventing the poisonous effects of fish; and strong liquors, wine, and spices are administered with the same intention to those who happen to be affected by poisonous fish. This fish poison seldom destroys life entirely, except the deadly poison of the yellow bill'd sprat, as it is called, which kills very speedily; but those who have eaten of the other kinds of poisonous fish, are frequently reduced to the last extremity by the vomiting, and life is almost extinguished before stimulants can take effect.

A singular effect of fish poison is to remove the epidermis in patches, or spots, about the hands and feet, which continue white in people of colour, and of a pale yellow colour in white people, for life.

The cause of some kinds of fish being poisonous, I suspect to be their feeding on submarine narcotic weeds, and not to their feeding on copperas banks in the sea, as hath been commonly supposed. The effects of the poison of the black land crab, which feeds upon the  
mountain

mountain Mahault, are exactly fimilar to thofe arifing from poifonous fifh.

The treatment of thofe who have taken a quantity of the Caffada juice, or eaten the root, is the fame as is generally practifed for all other narcotic poifons, viz. to empty the ftomach as fpeedily as poffible, and afterwards to adminifter the moft powerful cordials and ftimulants.

In cafes of this fort, when I have been called before the patient had loft the power of fwallowing, I have always given, to full grown robuft people, half a drachm or two fcruples of white vitriol, and lefs in proportion to fuch as were weakly, or to children. This dofe commonly operated in ten minutes, and fometimes fooner, if the patient had not become infenfible, in which cafe the dofe was repeated foon after. In every cafe I have feen, there was fuch a violent fpafmodic contraction of the mufcles of the throat, that it was with the utmoft difficulty any liquid could be got down. I have feen none die, however, who could fwallow.

After the operation of a vomit, ftrong ginger tea with fome rum in it, when wine could not be procured, was given in fmall quantities at a time, till the comatofe fymptoms (which proved con-

ly fatal when no internal remedy could be administered) were entirely removed. Some have been carried off by a strong convulsion; and large evacuations by stool were the forerunner of death. The stomach and bowels were always distended to their utmost extent, and the pupils of the eyes were much dilated.

I have opened the bodies of many who died of this poison, but could discover nothing remarkable, only that a great deal of very offensive air rushed from the stomach when it was opened, and the *Cassada*, or part of the juice was found in the bottom of it.

No cases of negroes being poisoned by this juice, have occurred to me since I found the *capficum* to be an antidote for it.

Cayenne pepper grows in great plenty upon all estates in this island, and on such as are at a distance from medical aid, and not provided with a vomit to administer directly, it ought to be tried. A quantity of it bruised in warm water and poured into the stomachs of horses, sheep, poultry, or stock of any kind, which have drank this juice, might be the means of saving them. No means hitherto tried for the recovery of animals that have drank it, have proved successful. *Capficum*, would, no doubt, relieve

relieve those who have eaten *farine* (or the root) not perfectly dried, by which the bowels are so much distended, as sometimes to endanger life.

When I was employed in making these trials on Cassada water, I also poisoned frogs, lizards, &c. with the juice of some other narcotic plants which are indigenous here, viz.

*Solanum mammosum*, Cock-roach Apple.

*Jatropha Curcas*, English Physic Nut.

———— *urens*, Prickly Nut.

*Datura ferox*, A species of Thorn Apple.

*Spigelia Anthelmia*, Worm Grass, a species of Indian Pink, called, by the French Inhabitants, *Brenvillié*.

The juice of the four first proved fatal to the frogs nearly as soon as that of the Cassada; but it is extremely acrimonious and therefore difficult to be given. The last is not so powerful a narcotic, and has been long in use as an anthelmintic. It differs from the *Spigelia Marilandica*, or Indian pink, described by the late Dr. Garden, of Charles Town, South Carolina, in the third volume of *Essays Physical and Literary*; the roots of the *Spigelia Anthelmia*, being so fibrous, that it cannot be reduced to powder; but the  
plant

plant possesses the same virtues in its leaves, seeds, and stalks, that the other species, or Indian pink, does in its roots.

The leaves boiled, or infused like tea, form a very powerful vermifuge, but when given in too large a dose, it has proved fatal to very young children, and it has on this account been laid aside for some time. Of the leaves and seeds, dried and pounded, from five to ten grains may be given, however, to a child above two years old without any risk; but the most common way of preparing this remedy for worms, is to make a strong infusion of the whole plant in boiling water; to which, a quantity of the rinds of four oranges, or lemons, is added, and some of the juice also; this is afterwards strained, and boiled to the consistence of syrup\*, with muscovado sugar. A table spoonful or two of this syrup given to a child from two to six years of age, thrice a day, for two days running, and a proper dose of *Ol. Ricini*, the

\* A handful of the plant to a gallon of water is the usual quantity. This is allowed to infuse, or rather simmer, for twenty-four hours, till one-fourth part of it has evaporated. The orange peel and acid are added, and a sufficient quantity of sugar to form it into a syrup, about an hour before it is taken off the fire.



day after, seldom fail to bring away a number of *lumbrici*, or round worms, to which children, particularly in warm climates, are remarkably subject. It is seldom given to children under two years old; and during its use, it is necessary to confine the patients in a dark room, as the light makes them quite giddy, the pupils of their eyes being dilated in an astonishing manner, and they feel a pain in the balls of their eyes. When given in too large doses it occasions dizziness, loss of sight, and slight comatose symptoms, which are removed by a spoonful or two of lime juice and water, or vinegar, and by washing the face with cold water. It seldom occasions sickness at the stomach; and when used in small doses, and the precautions above mentioned had been attended to, I have never known a single instance of its proving fatal, or even giving occasion for a serious alarm. It is rarely administered, however, until other vermifuge medicines have been tried without success, or until the case becomes very urgent and dangerous, owing to prejudices entertained against it. The number of worms that are discharged by the use of this syrup, in the space of two or three days, is almost incredible; they often amount to fifty or sixty, and sometimes to a hundred

hundred in that time. The syrup might be sent to England, but it would not keep long enough to be equally efficacious there. The dried leaves and seeds, however, might be sent and used in powder; in which state I have no doubt of its proving as powerful and safe a vermifuge, as it does in the form of syrup.

*Dominica,*  
*May 1, 1796.*

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XXVI. *An Account of some Experiments made with a view to ascertain the comparative Quantities of amylaceous Matter, yielded by the different Vegetables most commonly in use in the West India Islands. Communicated in a Letter to Dr. Simmons, by James Clark, M. D. F. R. S. Edin.*

**S**TARCH, or the amylaceous matter of vegetables, has justly been considered as their most alimentary or nutritive part. By the advice of my friend, Dr. Wright, of Edinburgh,  
 I un-

I undertook to make some experiments with a view to ascertain the comparative quantities of this substance in the roots or other parts of vegetables most commonly in use in the West-India islands.

I began with the roots of *Maranta arundinacea*, commonly called arrow-root.

A quantity of these roots, weighing four pounds avoirdupois, being first well scraped and cleaned, was grated or rasped, and about a gallon of water poured upon it and strained through a clean cloth. It was then left to settle for some time, and the water being poured off, fresh water was added, and so on, repeatedly, till it was quite clean. It was then exposed to the sun, to dry, for some days; and when perfectly dry, it yielded five ounces of very fine white starch. Similar quantities of other roots, &c. were treated exactly in the same manner, (care being taken to select for the experiments only such as were full grown) and after three trials of each, the medium products were as follows;

|                                                             | ozs. | drs. |
|-------------------------------------------------------------|------|------|
| 1. <i>Maranta arundinacea</i> , (arrow-root)                | 5    | 0    |
| 2. <i>Jatropha Janipha</i> , (sweet Cassida) not poisonous, | 13   | 6    |
| 3. <i>Jatropha</i>                                          |      |      |

|                                                                                                          | ozs. | drs. |
|----------------------------------------------------------------------------------------------------------|------|------|
| 3. <i>Jatropha Manihot</i> , (common bitter Cassada,) the water of which is poisonous, -                 | 10   | 6    |
| 4. <i>Dioscorea triphylla</i> (couch-couch, or yampee) -                                                 | 5    | 2    |
| 5. <i>Dioscorea bulbifera</i> , (Guinea yam) -                                                           | 8    | 0    |
| 6. <i>Convolvulus Batatas</i> , (West-India sweet potatoe) -                                             | 8    | 0    |
| 7. <i>Arum esculentum</i> , (Eddoes, white and yellow Tanners, Malingas, or Cocos) -                     | 11   | 0    |
| 8. <i>Musa paradisiaca</i> , (plantanes) full grown, (but not ripe,) peeled and grated, -                | 10   | 2    |
| 9. And by way of comparison, I tried <i>solanum tuberosum</i> , (potatoes) procured here from Ireland, - | 6    | 2    |

Four pounds of superfine Baltimore flour yielded two pounds of indifferent starch; but this was not a fair trial, as the husks ought to have been taken into the account; but no wheat is ever brought to the West Indies. On each of these I shall beg leave to add a few remarks.

1. The starch of the *Maranta arundinacea*, or arrow-root, is used as a light nourishment for

those affected with diseases of the breast or bowels, particularly in dysentery, lienteria &c. A tea spoonful of it mixed with a little col water, forms, with the addition of half a pt of boiling water, a mucilaginous drink, to which either milk or wine is added as may be found necessary; and it is rendered grateful to the palate and stomach by the addition of sug and nutmeg. Administered in this way, it answers the purposes of drink and nourishment, and often removes diseases proceeding from debility of the stomach and bowels, without the aid of medicines. It is never used as food for healthy people.

2. 3. The starch of the *Jatropha Janipa*, or sweet Cassada, answers the same purposes that of the *Maranta arundinacea* in every respect, and both have been found preferable to sago, in all cases where a lubricating, easily digested diet was indicated.

The starch of the *Jatropha Manihot* is equally nourishing as that of the *Jatropha Janipa*, but care must be taken to dry it very well as the juice is poisonous. By turning this starch with a flat piece of wood on a plate of iron (called a farine pan) well heated, the tapioca made, that has been so much recommended for its nutritive



tritive and restorative qualities all over Europe for many years past. I have made tapioca in this way, and used it; but the starch, when thoroughly dried, answers equally well, and is prepared with less trouble.

The roots of this poisonous plant, when full grown which is near a year after its being planted, are washed, and grated and turned, with a wooden spatula on a hot plate of iron till quite dry, by which operation very small grains called *trine* are formed. This is used as bread, by the planters and their negroes, in these islands and it is found to be extremely nourishing, and a very wholesome food. Cakes are also made of this farinaceous substance, upon the same plate of iron, which are eaten by all descriptions of people, and preferred by many to bread made of flour.

The *atropa fanipha* is not in common use; the chief reason of this is to prevent mistakes, as many negroes have been poisoned by roasting and eating the other species instead of this. From the great quantity of starch that it yields, it appears, however to be the most nourishing of the two, but it is not productive, having only a few small roots. It is distinguished from the bitter or poisonous one by a strong ligneous fibre in the middle

middle of each root, and is roasted or boiled when used at table, but never made into *farine*.

4. The roots of the *Dioscorea triphylla*, or couch-couch, are a very delicate food when roasted or boiled, and are thought to be equal to Irish potatoes; but they are used only by the white inhabitants, not being so productive as the other species of yams.

5. 6. 7. The *Dioscorea bulbifera*, or Guinea yam; *Convolvulus Batatas*, or sweet potatoe; and *Arum esculentum*, eddoes or coccos, (called *malingas* by the French;) are the chief support of many planters as well as of the negroes in the West Indies. The two first contain equal quantities of starch, and they are all three found to be remarkably nourishing when well boiled or roasted. The *arum esculentum*, or eddoe, makes the finest starch; and the roots boiled into a thick soup, and given to negroes, or others, affected with dysenteric complaints, prove to be a very salutary diet for them.

8. The *Musa paradisiaca*, or plantane tree, yields a fruit, which, when cut down green, and boiled or roasted, answers every purpose of bread, and may be styled with great propriety, the bread fruit of the West Indies.

It is a hearty food for hard-working people,

and when allowed to ripen, is a pleasant and grateful fruit. It contains a great proportion of starch, a tea spoonful of which made, with half a pint of boiling water, a thick mucilaginous kind of gruel, which had somewhat of the flavour of the ripe fruit, and was very grateful to the palate.

I tried to make starch from the *Musa Sapientum*, or Banana, when pulled green and full grown; but the water which was poured upon it became glutinous, and afterwards fermented and turned into vinegar. I tried to get starch from other fruits, but to no purpose. The common yam, when grated before it was full grown, turned the water into a mucilaginous state from which no starch was deposited; this also fermented and became vinegar. The *phaseolus*, so much in vogue in the islands of St. Domingo and Martinico some years ago, was planted and tried here, but the roots were very fibrous, or rather sticky, and did not contain much farinaceous matter, on which account the cultivation of this Plant was given up.

These are the chief roots made use of in this and most of the other West-India Islands; but there is a great variety of the leguminous class, which make a considerable part of the diet of the

inhabitants. These also contain starch, and are looked upon to be very nutritious. The saccharine matter which all fruits and vegetables contain, in a greater or less degree, is also very nourishing. The cane plant contains this salt or matter in the greatest proportion, and the nutritive quality of its juice is unquestionably very great; as the negroes in crop time, on sugar plantations, are observed to be constantly fat and healthy, and their chief nourishment, during that time, is known to be this juice raw or a little boiled.

The mucilaginous part of vegetables, particularly of the *hibiscus esculentus*, or okra, as well as of many other succulent plants, also contributes a little towards the nourishment and support of the inhabitants of these islands. All the West-India roots I have enumerated, when boiled or roasted, and pounded, may easily be converted into bread, if mixed with one half or two thirds the quantity of flour, and afterwards fermented with leaven.

Whether the starch produced from the Irish potatoes will be considered as a fair comparative trial, I shall not take upon me to determine. I took some pains, however, to select the best that could be procured for these experiments,

but found that the same weight yielded less starch on every trial than the West-India roots, those of the *Maranta arundinacea*, and *Dioscorea triphylla* excepted. Perhaps this may be attributed to their having been long kept; for there can be no doubt of potatoes being possessed of extraordinary nutritive qualities: I cannot help thinking, however, that the inhabitants of these islands are at no loss for substitutes equally wholesome, and as nourishing.

*Dominica,*

*May 21, 1796.*

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**XXVII.** *A fatal Instance of the poisonous Effects of the Oenanthe Crocata Linn. or Hemlock Dropwort. Communicated in a Letter to Dr. Simmons, by Robert Graves, M. D. Physician at Dorchester, and Extra Licentiate of the College of Physicians, London.*

**A**MONG the numerous poisonous plants with which the vegetable kingdom  
 I  
 abounds,



abounds, there are few, perhaps, natives of this country, more virulent and speedy in their effects, than that which gave birth to the following melancholy incident; in confirmation of which, indeed, too many fatal examples are to be found, scattered in the writings of medical men. Against so destructive an enemy to mankind, therefore, it behoves us to be particularly on our guard; and every unhappy instance wherein the contrary takes place ought not, in my opinion, to be suffered to pass wholly unrecorded. It is only from being made acquainted with the errors or defective caution of others, that we, in many cases, become mindful to keep clear of such ourselves.

A young woman, about twenty-four years of age, servant to a lady in this neighbourhood, having been recommended by a medical practitioner of eminence, to take the juice of the water-parsnep for an eruptive complaint in her face, became exceedingly desirous of trying it in the month of May last. The gardener of the family was applied to, and earnestly entreated by her to collect the plant; but from its not being sufficiently known to him, he at first hesitated, very properly, to comply with her request. It happened, that in a moist watery

ditch or brook, within a short distance of the house where she lived, a large quantity of hemlock dropwort was growing. This unfortunately she discovered, and conceiving, both from its smell and appearance, to be the herb which she was in quest of, she communicated the same to the gardener, with an express declaration, that if he would not gather some for her, she would undertake to do it herself. The gardener being thus forcibly solicited, and not aware of the virulent and deadly nature of the plant which he was about to furnish her with, was at length brought to compliance.

On the morning of the 19th of the same month, being plentifully supplied with the whole plant, she took the roots of it, bruised them herself, and expressed from them, as it was supposed, juice to the amount of two table spoonfuls at least, which she swallowed. Scarcely had more than ten or fifteen minutes elapsed afterwards, before she complained of great giddiness, accompanied with vast uneasiness and sickness at stomach, though not such as to be succeeded by vomiting; convulsions likewise speedily supervened, and she became entirely deprived of her senses.

It was about half an hour after this, when I

was

was called to her assistance. At that period the convulsive paroxysms were become not only stronger, but recurred in quicker succession; all sensibility was destroyed; the mouth firmly open, with scarce any pulse to be felt at the wrist, and a coldness had seized the extremities. Prior to my seeing her, repeated draughts of milk and oil had been got down, agreeably to the advice of Mr. Arden, a judicious and experienced surgeon of this place, but without the least apparent effect, or sickness, succeeding. Wishing, therefore, if possible, notwithstanding the truly hopeless state of the case, to solicit a discharge of the poison as the most effectual step towards affording relief, an emetic was immediately sent for; but though it arrived in the short space of ten or twelve minutes, she did not survive even to take it. Besides the repeated draughts of milk and oil above mentioned, between two and three ounces of vinegar also were forced down, at my direction, from an idea that this acid might act somewhat as a corrective.

I shall close this case with a very necessary caution, and which I think cannot be too strongly impressed on the minds of medical men. Though that species of the water-parsnep,

(*Sium nodiflorum* Linn.) directed by the London college, bears but little resemblance indeed to the hemlock dropwort; yet from the fatal error here related, joined to a similar one, which happened also in this county but a few years since\*, practitioners, I conceive, ought on no account, whenever they should deem it proper to recommend the former plant, to trust to unskilful people to gather it. Both plants are to be found frequently inhabiting the very same moist, watery situations; and the ignorant and unwary are as likely to be imposed upon by the latter, as to have their choice determined, or attention drawn by the former.

*Dorchester,*

October 21, 1796.

\* London Medical Journal, Vol. 5, p. 192.

## CATALOGUE OF BOOKS.

1. **A** TREATISE upon Gravel and upon Gout, in which their Sources and Connection are ascertained; with an Examination of Dr. Austin's Theory of Stone, and other Critical Remarks. A Dissertation on the Bile, and its Concretions; and an Inquiry into the Operation of Solvents. By *Murray Forbes*, Member of the Surgeon's Company. 8vo. *Cadell*, London, 1793.

2. A Case of Hydrophobia, commonly called Canine Madness, from the Bite of a Mad Dog, successfully treated. By *Thomas Arnold*, M. D. Fellow of the Royal College of Physicians and of the Royal Medical Society of Edinburgh. 8vo. *Dilly*, London, 1793.

3. The Art of preventing Diseases, and restoring Health, founded on Rational Principles, and adapted to Persons of every Capacity. By *George Wallis*, M. D. 8vo. *Robinsons*, London, 1793.

4. Advice to Parents on the Management of Children in the Natural Small Pox. 8vo. *Robinsons*, London, 1793.

5. A Trea-



5. A Treatise on the Errors and Defects of Medical Education; in which are contained Observations on the Means of correcting them. By *Thomas Withers*, M. D. M. M. S. L. Physician to the York County Hospital. 8vo. *Dilly*, London, 1794.

6. An Essay on the Management, Nursing, and Diseases of Children, from the Birth; and on the Treatment and Diseases of pregnant and lying-in Women: with Remarks on the domestic Practice of Medicine. The second Edition revised and considerably enlarged. To which is now added, the Treatment and Diseases of Children at more advanced Periods of Childhood; with Observations on Mothers nursing their Children. By *William Moss*, Surgeon to the Liverpool Lying-in Charity. 8vo. *Longman*, London, 1794.

7. An Account of a new and successful Method of treating those Affections which arise from the Poison of Lead; to which are added general Observations on the internal Use of Lead as a Medicine. By *Henry Clutterbuck*, Member of the Corporation of Surgeons, &c. 8vo. *Boosey*, London, 1794.

8. Letters, concerning the internal Dropfy of the Brain, to Charles William Quin, M. D.  
Fellow

Fellow of the King's and Queen's College of Physicians, Physician General of His Majesty's Army in Ireland, and of the Royal Hospital for Invalids, near Dublin, from *William Patterson*, M. D. Member of the Royal Irish Academy, and Corresponding Member of the London Medical Society. 8vo. Dublin, 1794.

9. Remarks on some of the Opinions of Dr. Rush, respecting the Yellow Fever which prevailed in Philadelphia in 1793. By *William Patterson*, M. D. 8vo. Londonderry, 1795

10. A Dissertation on Simple Fever, or on Fevers consisting of one Paroxysm only. By *George Fordyce*, M. D. F. R. S. Senior Physician to St. Thomas's Hospital. 8vo. *Johnson*, London, 1794.

11. A Second Dissertation on Fever, containing the History and Method of Treatment of a regular Tertian Intermittent. By *George Fordyce*, M. D. F. R. S. &c. 8vo. *Johnson*, London, 1795.

12. An Address to Medical Students; a Letter to Dr. Fordyce; with Remarks and Questions upon Quotations from Dr. Fordyce's Dissertation on Simple Fever. 8vo. *Bell*, London, 1795.

13. The Physician's Vade Mecum; being a Compendium of Nosology and Therapeutics,  
for

for the Use of Students. By the Rev. *Joseph Townsend*, Rector of Pewsey, Wilts; and Author of a Tour through Spain. 12mo. *Dilly*, London, 1794.

14. A Guide to Health; being Cautions and Directions in the Treatment of Diseases. Designed chiefly for the Use of Students. By the Rev. *Joseph Townsend*, Rector of Pewsey, Author of the Physician's Vade Mecum, and of a Journey through Spain. 8vo. *Cox*, London, 1795.

15. Catechism of Health: for the Use of Schools, and for Domestic Instruction. By *B. C. Faust*, M. D. Counsellor and Physician to the Reigning Count of Schaumburgh Lippe, &c. translated from the last improved German Edition of this Work by *J. H. Basse*. 12mo. *Dilly*, London, 1794.

16. The rational and improved Practice of Physic. By *William Rowley*, M. D. Member of the University of Oxford, the Royal College of Physicians in London, and Physician to the St. Mary-le-Bone Infirmary. 4 vols. 8vo. *Newbery*, London, 1794.

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\* See vol. IV. p. 207.

concisely stated, their Pronunciation as to Quantity is distinctly marked, and a Variety of other Particulars respecting them given; calculated more especially for the Use of Junior Practitioners. By *Robert Graves*, M. D. of the Royal College of Physicians, London; Member of the Medical Societies of London and Edinburgh, &c. 12mo. *Murray and Highly*, London, 1796.

118. Transactions of the Linnean Society. Vol. II. 4to. *White*, London, 1794.

119. A Botanical Nomenclator, containing a systematical Arrangement of the Classes, Orders, Genera, and Species of Plants, as described in the new Edition of Linnaeus's *Systema Naturæ*, by Dr. Gmelin, of Gottingen. To which are added, alphabetical Indexes of the Latin and English Names of the Plants, together with the Names of the Countries of which they are Natives; also the Number of British Species. By *William Forsyth*, junior. 8vo. *Cadell*, London, 1794.

120. A Supplement to Medical Botany; or Part the Second: containing Plates, with Descriptions of most of the principal medicinal Plants not included in the *Materia Medica* of the Collegiate Pharmacopœias of London and Edinburgh; accompanied with a circumstantial Detail of their medicinal Effects, and of the Diseases in which they have been successfully employed.

employed. By *William Woodville*, M. D. F. R. S. Physician to the Small-Pox and Inoculation Hospitals. 4to. *Phillips*, London, 1794. With 64 plates.

121. The Natural History of Aleppo. Containing a Description of the City, and the principal natural Productions in its Neighbourhood: together with an Account of the Climate, Inhabitants, and Diseases; particularly of the Plague. By *Alex. Russell*, M. D. The Second Edition, revised, enlarged, and illustrated with Notes. By *Patrick Russell*, M. D. and F. R. S. 2 Vols. 4to. *Robinsons*, London, 1794.

122. Plants of the Coast of Coromandel, selected from Drawings and Descriptions presented to the Honourable Court of Directors of the East-India Company, by *William Roxburgh*, M. D. Published by their Order, under the Direction of Sir *Joseph Banks*, Bart. P. R. S. Folio. Fasciculi I. II. III. *Nicol*, London, 1795-6.

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124. Hortus Botanicus Gippovicensis; or a systematical Enumeration of the Plants culti-

\* By *John Aikin*, M. D.



vated in Dr. *Coyte's* Botanical Garden at Ipswich, in the County of Suffolk; also their generic Characters, English Names; the Natives of Britain particularized; the Exotics, where best preserved, and their Duration; with occasional Botanical Observations. To which is added, an Investigation of the Natural Productions of some Grass lands in High Suffolk, 8vo, Ipswich, 1796.

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11. *Hier. Mercurialis nomothelasmus, five ratio lactandi infantes*. This last, which was first published at Padua in 1552, is here reprinted, it seems, on account of its scarcity. EDITOR.

173. Episc.

173. *Epistolæ Halleri ad Levelingium scriptæ*, quas edidit, præfatus est, notisque illustravit *D. H. M. de Leveling* filius, S. R. I. Eques, Conf. Elect. Aët. Almæ Universitatis Ingolstad. Anat. Physiol. Diætet. et Anthropol. Prof. p. o. &c. 8vo. Erlangæ, 1795.

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4to. Goettingæ, 1795.

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